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ON THE FRESH WATER ALGÆ AND THEIR RELATION TO THE PURITY OF PUBLIC WATER SUPPLIES.

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WITH DISCUSSION.

It has been my pleasure to devote the most of my leisure for the last few years to studying questions relating to the preservation and improvement of public water supplies. During this time I have pursued, in a desultory way, some lines of investigation which, I hope, may be of interest to the members of the Society, and which I venture at this time to briefly lay before you.

Knowledge of the sanitary significance of many of the minute forms of plant life inhabiting our water supplies is very limited, and it is only when some special reason exists that students direct their attention to the sanitary side of the question. This is the more remarkable when we consider the amount of energy that has been expended in study from the purely botanical point of view.

Take, for instance, the fresh water algæ, to which the present paper is particularly devoted. Including the desmids, but not the diatoms,

about fifteen hundred species have been illustrated and described as peculiar to this country, while in Europe, where more complete studies have been made, a much larger number are known to cryptogamic botanists. Moreover, this number is being added to from year to year, so that he who studies the fresh water algæ can hardly fail to have new species to determine, whenever he sees fit to go forth into the fields and search diligently for them.

Much misconception has existed as to the cause of the various bad tastes and odors, known as "fishy," "pig-pen," "cucumber," "musty," "woody," and others, with which different water supplies have been affected. The impression is quite general, not only that the plants producing these different tastes and odors are limited in number, but that the grass-green varieties of algæ are entirely innocent. Both views are undoubtedly erroneous, as it is my experience that very many of the algæ will in an experimental way produce objectionable tastes and odors, and that some of these are especially virulent in the case of certain of the grass-green varieties. Both of these points have been tested by experimenting on at least two hundred species, representing not only my own collections for three years, but a large number of samples received from correspondents.

Before proceeding to discuss some of the more interesting results of the study, I will call attention to a few peculiarities of the fresh water algæ in relation to habitat.

The algæ are in the fullest sense hardy plants, many of them growing vigorously in the middle of winter. This I have had occasion to notice, particularly during the past winter, when a form resembling *Microcoleus*,* an alga genetically related to *Nostoc*, has been exceedingly plentiful in the domestic water supply of the city of Rochester, without, however, so far as known, being the cause of any trouble. This development was at its height in the latter part of the winter (February 20th), when Hemlock lake was covered with an unbroken sheet of ice from twelve to fifteen inches in thickness.

This abundant vitality was even more markedly forced upon my attention about March 1st of the present season (1889), while doing field work in Livingston county, N. Y., where I observed the ground not only partly covered with melting snow, but, for a considerable area, with a vigorously germinating alga. In the absence of a microscope its species

* Considered to be the agglutinated form of *anabæna*.

could not be determined, and I am able therefore only to record the fact of having seen, while the snow still largely covered the ground, the visible signs of such a development.

The observation had to me greater interest than would usually attach, from the fact that only a few days before seeing it the temperature had fallen several degrees (8° to 12°) below the zero of the Fahrenheit scale.

It has been well known for some time that numerous species of algæ flourish in the arctic regions of perpetual snow, and it is of interest in this connection to cite that a study a few years since of the algæ of Nova Zembla shows the existence of one hundred and seventy-two fresh water species belonging to fifty-seven genera.*

Chlamydococcus pluvialis, the well-known red snow† of the arctic region, has an extended range of habitat, being found plentifully in even New York and Pennsylvania. It must be admitted, therefore, that low temperature does not necessarily imply cessation of growth of all of the algæ, and we must extend our limit of vision in this direction also. Indeed, high temperature will undoubtedly be as fatal to some species as low temperature would be to those which are peculiar to thermal springs.

While it is thus true that many species are hardy, it is equally sure that some of them are at certain stages of development extremely sensitive to changes in the environment—a fall of temperature of a few degrees, a slight change in the direction of the wind, humidity of the atmosphere, or variations in the chemical constituents of the waters they inhabit, frequently sufficing to cause their immediate disappearance from localities the most favorite.

Again, certain species only live in waters containing some particular substance in solution, as, for instance, *Beggiatoa* in waters containing sulphur in some form, *Batrachospermum moniliforme* in rapid running limestone waters, while some species of *Inactis* are at home only in sandstone waters. *Cladophora* and *Vaucheria* are primarily marine forms, and it is doubtful if the fresh water varieties will continue to exist in a water absolutely free of sodium chloride in solution.

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* Jour. Roy. Micr. Soc., 1880, page 679.

† Cooke's British Fresh Water Algæ and Wolle's Fresh Water Algæ of the United States contain full description. Both agree that *C. pluvialis* and *C. nivalis* are probably one and the same alga.

Others affect sluggish streams and stagnant ponds, some modestly hiding under shaded banks and beneath water-logged trees, while others again prefer the full light of noonday, and boldly seek open spaces in the purest waters. They are found at all depths, from nothing to the bottom of the deepest lakes. In the somewhat crude experiments which I have made I have endeavored to reproduce, so far as I could, in my workroom, with an assortment of bottles of various sizes, beaker glasses and fruit jars, the natural conditions of growth of each species. In many cases I have had only failure as the result; they utterly refused to grow, but in their death I gained information as to the odor-producing capacity to be gained in no other manner.

Pursuing the line above indicated relative to the grass-green varieties, it may be generally stated that the recent studies indicate the ability which these minute cryptogams possess of causing various bad tastes and odors to be entirely independent of color. This is emphasized by considering that *Beggiatoa* and *Crenothrix* are in effect colorless, *Cladophora* and *Volvox* grass-green, while *Clathrocystis* and *Nostoc* are bluish green. It seems equally certain that, so far as present systems of classification are concerned, we cannot refer the power to produce these difficulties to any particular class or order of cryptogams. We must look farther for the final cause. This is usually found in the jelly, starch, oil or sulphur-producing capacity of these plants, and generally the question of whether or not any particular species is likely, under favorable conditions, to produce trouble, will be settled by observing whether it develops at any stage of its life history an albuminous gelatine, either as an essential part of cell structure, or as an enveloping matrix, or whether it secretes starch, oil or sulphur at some period of its existence. An observation as to which of these is the predominant characteristic of any given alga will also indicate the probable quality of any tastes or odors of which it may be the cause. The intensity of the resulting tastes and odors will also be indicated by the vigor of development and the quantity present in any given case.

So far as I am aware, no one has yet advanced a theory accounting fully for these various bad tastes and odors, and it is with considerable diffidence that I venture to offer the foregoing, which can only be called a working theory, subject, I have no doubt, to modification, as more complete studies are made of this interesting and very practical question.

It is well known that certain algæ contain starch grains in more or less quantities in connection with the chlorophyl granules.

Cladophora in particular is distinguished by the possession of a large number of such grains.* The presence of these starch grains may be demonstrated by the application of the iodine test,† though in many cases, when one knows just what to look for, the starch can be distinguished without the application of any special tests.‡

About two years ago I found in one of my gatherings a specimen of *Cladophora* containing an unusually large amount of starch throughout the chlorophyl, so much so that I began to make an extended study of the specimen. An interruption of a few days occurred, and when I resumed the study I found the specimen well advanced in decay and producing a nearly unbearable stench. I thereupon experimented with a solution of starch and water, and found it to give off, after standing for a few days, a quite similar stench.|| With this as a basis I began to gather all the species of algæ containing starch that I could find, and by experiment under various conditions endeavored to ascertain whether any other species containing starch gave objectionable odors on decay, and, while I have by no means completed the study, the result thus far seems to justify saying, in a general way, that all the chlorophylaceous algæ, which contain either starch grains or amylaceous granules in the chlorophyl, are likely, if they develop in quantity, to produce at some stage an unpleasant odor in decay.

Again, many of the algæ secrete globules of oil in connection with the chlorophyl. *Vaucheria* in particular, with its green, robust filaments, has its chlorophyl rather evenly distributed on the inside of the walls of the tubes, with green granules and minute oil drops imbedded.§ *Vaucheria sessilis* has this development of oil drops especially well marked, though this alga will not usually be the cause of trouble with water

* See description of *Cladophora* in Wolle, *loc. cit.*

† For detail of iodine test for starch in chlorophyl grains, see Behrens' Guide to the Microscope in Botany, Hervey's translation, page 364.

‡ Two forms of grain or granule may be distinguished, the starch grain pure and simple, and the amylaceous granule which resembles starch, and which may or may not actually become starch, depending upon phases of development. The following is given as a partial list of fresh water algæ which exhibit one or the other of these forms: *Chætomorpha, Cladophora, Conferva, Cylindrocapsa, Microthamnion, Spirogyra, Ulothrix, Vaucheria, Volvox, Zygogonium, Zygnema.*

|| The water from starch factories becomes particularly offensive. See paper by the late Professor William Ripley Nichols, Some Remarks on the Tastes and Odors of Surface Waters, in the Journal of the Association of Eng. Societies, for January, 1882.

§ So described by Mr. Wolle, *loc. cit.*

supplies, as its usual habitat is wet ground, except that it is occasionally found in ponds and ditches; but what is true of one species is likely to be true of others, and I cite *Vaucheria* in this connection because it is widely distributed and easily obtained for purposes of study. There are a number of species of *Vaucheria*, and one of these, *Vaucheria dichotoma*, is only found in marshy places in the vicinity of limestone springs.

The late Professor Nichols records a statement of Mr. Fteley* that the addition of common salt to water possessing the cucumber taste will sometimes develop a decided oily flavor. I have no doubt but that this observation is correct, and that some of the oil-secreting algæ are responsible for the so-called cucumber tastes, which possibly are modified by the addition of the common salt, while at the same time that of the oil is accented enough to bring it markedly within the range of the sense of taste.

There are, however, a large number of species of algæ in which the starch grain and oil globule are not the most striking features of development, and these are the species which are generally credited with causing all the trouble. A number of them belong in the class *Cyanophyceæ* and the order *Schizosporeæ*, including among such *Nostoc*, *Clathrocystis*, *Cœlosphærium*, *Lyngbya* and others. A number of the cryptogams of this class and order are inclosed in a maternal jelly, a typical form of which is well illustrated by *Nostoc*. We have, however, *Batrachospermum*, in which the jelly mass is an integral part of the development, in the class *Rhodophyceæ*, while among the class *Chlorophyceæ* and in the order *Confervoideæ* may be found *Draparnaldia* and *Chætophora*. Also in the order *Protococcoideæ* we find *Volvox*, *Eudorina*, *Pandorina*, *Hydrodictyon* and *Palmela*. All of these are likely by reason either of the jelly matrix or the gelatinous structure, to give rise, when they develop in quantity, to objectionable tastes and odors in water supplies.

In order to complete this portion of our working theory it is desirable to indicate the relations which subsist between the production of the jelly mass, as an integral part of the life history of an alga, and the processes of assimilation and nutrition. This will lead us to consider, briefly, not only the formation of the chlorophyl, but also the vegetable jelly or mucilage. The fundamental substance of chlorophyl is nitrogenous matter, and it is according to Sachs† a protoplasmic form.

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* In Some Remarks on the Tastes and Odors of Surface Waters, *loc. cit.*

† Text Book of Botany.

In the process of development there are stored up assimilated substances, called reserve material. This at the proper time is drawn upon, and through the process of nutrition finally becomes part chlorophyl, part cell membrane and part refuse matter, this refuse matter, which is rejected as waste material, taking the form of jelly.* Such in a few words is probably the origin of the vegetable mucilage or enveloping jelly of many of the algæ. In any case it is essentially a form of protoplasm, nitrogenous in its chemical constitution and liable in decay to produce offensive odors, as indeed it is well known to do from the observed facts.

A few of the algæ of the class under consideration, however, do not secrete any exterior mucilaginous or gelatinous matrix, as, for instance, *Volvox globator*, which has usually been considered a hollow sphere, but is now known to be exactly the contrary, the sphere being entirely filled with a comparatively solid mucilaginous jelly.† In this the young *Volvoces* develop very much as *Nostoc* develops in its enveloping matrix of albuminous gelatine.

Hydrodictyon utriculatum grows abundantly on the limestone rocks forming the bed of the Genesee river, in the city of Rochester. This stream has so low a summer flow that, occasionally, its bed between Main street and the Johnson and Seymour dam is nearly dry for several days at a time, by reason of the mill races at either side taking the entire flow of the river. On such occasions there frequently arises from the river bed an odor which is an offence to every one in the vicinity. On one occasion, when the odor was especially rank, I ventured into the river bed, and securing specimens of the abundant growth of *Hydrodictyon*, easily satisfied myself as to the important part which this alga played in its production. At this time the growth of *Hydrodictyon* was so abundant that several wagon loads could have been easily gathered from a few hundred feet of river bed. The common name of *Hydrodictyon* is "water net," by reason of its growing in a net-like series of reticulations, frequently to the length of from ten to twelve inches. I have never seen it in any waters except those containing considerable organic impurity.‡

* The basis of this view may be found in Sachs' Text Book of Botany, Chapter on Chemical Processes in the Plant. The view there advanced by Professor Sachs is a perfectly general one, applying quite as well to cryptogamous as to phenogamous plants.

† On *Volvox globator*. Is it a Hollow Sphere? Jour. Roy. Micr. Soc., 1883, page 889.

‡ See Wood's American Fresh Water Algæ for complete account of development of *Hydrodictyon*.

The ground taken by some of the earlier students of this subject, that the diatoms never cause trouble, must, I think, be modified in the light of recent experience. These little cryptogams, while possessing an incombustible, silicious frustule, have in addition not only a thick, gelatinous enveloping membrane, which, when they multiply in quantity, will undoubtedly produce the tastes and odors referred to, but they also produce starch in the chlorophyl. During last summer I made, on several occasions, gatherings, about the margins of Hemlock lake, the source of the domestic water supply of the city of Rochester, which, even when freshly gathered, gave a vile pig-pen odor, and which on examination with the microscope proved to be composed almost entirely of diatoms; and I have been informed by J. Nelson Tubbs, M. Am. Soc. C. E., Chief Engineer Rochester Water Works, that, on one occasion several years ago, a similar deposit was so extensive as to cause an unpleasant odor for several hundred feet from the lake. At this time the water itself was entirely unaffected, the odor proceeding, evidently, from the decay of the protoplasmic part of the diatoms scattered along the beach.

J. D. Hyatt has described an extraordinary development of the common diatom, *Meridion circulare.** This diatom has a thick, gelatinous envelope, and, early in the spring of 1881, developed to such an extent in the headwaters of the Croton river as to cover for many miles every submerged object in the streams to the depth of nearly a quarter of an inch. In about twenty days after its first appearance this growth began to break loose and in a week it had entirely disappeared, but following the disappearance of this sporadic growth of *Meridion* in the upper Croton, the water as delivered in New York began to be pervaded by an unpleasant odor, which it is fair to assume was due to the decomposition of the gelatinous envelope of *Meridion.*† This continued for about two weeks, and Mr. Hyatt states it to have been in all respects exactly similar to that of the odor from the decomposing specimens contained in his collecting bottles; and he further remarks that on experimenting it was quite surprising to observe how small a quantity of the fetid gelatinous matter was sufficient to taint a large volume of water.

* On Sporadic Growth of Certain Diatoms, by J. D. Hyatt, in the Transactions of the American Society Microscopists, 1882, page 197.

† The trouble was so serious as to lead to the employment of experts to examine the Croton water. Such examination made from samples taken in the city resulted in a report that Croton water contained no unusual amount of organic matter, and nothing in any way deleterious to health.

So far as known the desmids have never been the cause of any trouble in public water supplies. This I apprehend is chiefly because they are less liable to sporadic growth than the diatoms and filamentous algæ, for they certainly possess characteristics which would lead to the production of tastes and odors if they developed in quantity. They are all more or less gelatinous and some species have a distinct, wide, colorless envelope.*

Crystals of sulphate of lime are found in the terminal vesicles of *Closterium* and other desmids, and the presence of these crystals suggests the agency of the desmids in the reduction of foreign matter in a manner similar to the production of oxalate of lime in *Spirogyra* and other species of filamentous algæ.†

A. Fteley, M. Am. Soc. C. E., records in the Supplement to the Report of the Massachusetts State Board of Health for 1879‡ the results of a number of observations on the development of algæ in some of the basins of the Boston Water Works. Among many other interesting facts he states that sulphureted hydrogen was present in basin No. 3 in such quantity as to be plainly perceptible to the sense of smell while the water was passing through the sluice gates. It was also observed, not only that algæ were numerous in the basin at this time, but that the temperature was uniform throughout the whole body of water, even to the depth of twenty feet. Mr. Fteley suggests that this uniformity of temperature was produced by chemical action, without, however, giving any clew as to the process by which the temperature was kept uniform. It appears, however, that the algæ were probably concerned in the matter in about the following way. It has been already mentioned that *Beggiatoa* only exists in waters containing sulphur in some form; and the further fact is now advanced that it and certain other algæ possess the power of decomposing sulphates in waters containing them in solution, a portion of the sulphur being absorbed in an uncrystallized state into the protoplasmic mass of the cell structure, while the balance goes to form sulphureted hydrogen as a free product. Waters containing either sulphates of lime or soda in solution are liable to this trouble.

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* Wolle's Desmids of the United States, page 15.

† On the Occurrence of Crystals of Gypsum in the Desmidieæ. Jour. Roy. Micr. Soc., 1884, page 108.

‡ Algæ Observed in Storage Basin No. 3 of the Boston Water Supply in 1879, by A. Fteley, C. E., Res. Eng.

A. Etard and L. Olivier have studied this question broadly,* and they conclude, in addition to *Beggiatoa*, which is nearly always found abundantly in sulphurous waters, that *Oscillaria*, in certain stages which they detail, and *Ulothrix*, present similar phenomena, and it is probable from some incomplete observations by the present writer that several other species possess this property in a more or less limited degree.

The observations of Etard and Olivier, however, extended only to the power of these algæ to decompose the mineral sulphates. Nevertheless, it is probable that *Beggiatoa* at any rate has the power not only of extracting sulphur from the mineral sulphates, but it can also extract it from decomposing organic matter, when containing it, so that the presence of sulphureted hydrogen in any given water supply may or may not be a danger signal, according to circumstances. It appears easy, therefore, to assent to Mr. Fteley's observation as to the nature of the action producing the same temperature throughout a large body of water twenty feet in depth.

The late Professor William Ripley Nichols explained the presence of sulphureted hydrogen in basin No. 3, referred to by Mr. Fteley, in a manner somewhat different from the foregoing.† According to Professor Nichols, the flooding of the basin started the decay of a large quantity of organic matter; this taking place in the presence of the sulphates contained in the water, changed them into sulphides, and from these sulphides thus formed sulphureted hydrogen was liberated by the acid products of decay. It appears to me, however, that if this is the whole state of the case, the chemical action must have been confined almost entirely to the sides and bottom of the basin, in consequence of which currents must have been produced, leading to varying temperatures in different parts. The fact, however, that the temperature was not only high, but uniform at all depths and localities, would indicate a distribution of the chemical action. The presence of large quantities of algæ, which in the absence of any evidence to the contrary we may assume as acting at least as assistants, would furnish a rational explanation without doing violence to any of the observed phenomena.

This property of decomposing the sulphates renders *Beggiatoa* of considerable interest to sanitarians, and although it has received a large

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* On the Reduction of Sulphates by Algæ, in Jour. Roy. Micr. Soc., 1883, page 259, from *Comptes Rendus* XCV (1882), pages 846 to 849.

† Some Remarks on the Tastes and Odors of Surface Waters, *loc. cit.*

amount of attention,* its position in a systematic classification is not yet well defined. Mr. Wolle, writing as an algologist purely, is disposed to reject the whole genus, referring it rather to *Leptothrix*. The bacteriologists, however, retain the genus and place it among the *Schizophytes*. Cohn's classification, which is the best known, places *Leptothrix*, *Crenothrix*, *Beggiatoa*, *Hypheothrix* and *Oscillaria* in close relation in the second section of the *Schizophytes*, or among the *Nematogenes*.† It is probable that *Beggiatoa* has bacillus, coccus and spirillum phases of development, and this, if true, would justify its classification with the bacterial forms.‡

It has been assumed by the promoters of ground water systems of water supply that the water so obtained would be entirely free from all the various troubles which, in a general way, may be said to be due to the presence of organized life; especially has it been claimed that such supplies could by no possibility contain the algæ, and, in consequence, immunity from the various tastes, odors and colors characterizing surface waters has been confidently predicted. This claim, however, is fallacious, as even ground waters are found to have difficulties of this sort also, and as certain ground water supplies of this country have recently passed through such trouble, it is considered that a general account of a similar trouble which has been very thoroughly studied abroad will be of interest in the present connection.

The trouble referred to was at Berlin, where, since 1877, a part of the supply has been drawn from a series of covered wells in the vicinity of the Tegel lake. The water is taken from the wells and pumped to covered reservoirs, from whence it passes into the city distribution without exposure to light or open air at any point of its passage. Soon after the introduction of this water it was found to be offensive, not only to taste and smell, but to the sense of sight as well. When first drawn from the wells it appeared tasteless and colorless, but, on standing, became tur-

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* See Fungi found in Sewage Effluents, paper by A. W. Bennett, in Trans. Am. Soc. Micr., 1884, page 90.

† For Cohn's Classification see The Bacteria, by Dr. George M. Strenberg.

‡ The following will give full reference to *Beggiatoa:*

(*a*) Micro-organisms and Disease, by E. Klein, 3d Ed. (1886), page 113.

(*b*) A. W. Bennett, *loc. cit.* Also note on Sewage Fungus, in Jour. Roy. Micr. Soc., 1884, page 927, from Proc. of Brit. Assoc. Adv. Sci. (Montreal), 1884.

(*c*) Manual of Bacteria, by Edgar M Crookshank, 2d Ed. (1887), page 324.

(*d*) Lectures on Bacteria, by A. De Barry, 2d Ed., translated by Henry E. F. Garnsey. Numerous references.

(*e*) Wolle, Fresh Water Algæ of United States.

bid, and deposited a yellow sediment, consisting of oxide of iron, amorphous matter, and what on microscopical examination was found to be an alga with numerous specimens, both living and dead. This alga was *Crenothrix Kühniana* or *Crenothrix polyspora*, Cohn. Rabenhorst* gives it *Leptothrix Kühniana* and *Hypheothrix Kühniana*. Professor Kühn originally discovered *Crenothrix* in Silesian farm drains, where its development was so rapid as to completely choke the drains in a few days. It was also studied by Professor Cohn, and finally at Berlin by Dr. Zopf, whose paper† is a complete account of all stages of its life-history.

In decay this plant gives rise to the most offensive tastes and odors, so that Dr. Zopf's title, *Crenothrix polyspora* die Ursache der Berlin *Wassercalamität*, can be taken as expressing the consternation of the people of Berlin at the apparently serious calamity which had fallen upon their water supply.

On investigation it was found that *Crenothrix* was present in the wells, reservoirs, force mains and distribution pipes, in various stages of development and decay. The spores (gonidia) are from $\frac{1}{25000}$ to $\frac{1}{100000}$ inches in diameter, and after passing through a series of developments, which Dr. Zopf has thoroughly illustrated in the three elegant plates accompanying his paper, finally attain to the condition of long filaments, which under the microscope are found to be a series of individual cells enclosed in a gelatinous sheath. Figure 1, Plate IV, illustrates the appearance of a typical mass of these filaments in an early stage of development.

In the beginning these filaments are transparent and colorless, but they finally become olive or brown by absorption of iron. Frequently they become so incrusted with iron as to render their structure invisible (see Figure 1, Plate V), but the application of dilute muriatic acid will dissolve the iron, again revealing the structure.

The investigation also showed that *Crenothrix* developed and passed through the various stages of its life-history in the ground itself, and consequently without access to light. This was proven by sinking a number of driven wells to various depths in the vicinity of the supply wells and the taking of water therefrom for examination under such con-

* In *Flora Europæa Algarum Aquæ dulcis et submarinæ*, by L. Rabenhorst.

† *Entwickleungsgeschlichtliche Untersuchung uber Crenothrix Polyspora die Ursache der Berlin Wassercalamitat.* By Dr. W. Zopf.

ditions as precluded the possibility of error. In some cases the plant was found more than 75 feet below the surface.

It also appeared from Dr. Zopf's study that iron in solution is necessary for the existence of *Crenothrix*, as even in the early stages of development, when the filaments are quite colorless, the application of potassium ferrocyanide will turn them blue, thereby indicating the presence of iron, while, as already stated, in the later stages the reducing or transforming action becomes so energetic as to completely incrust the filaments to the extent of concealing the structure. Lime, however, it is said, destroys it. At Halle, where *Crenothrix* also gave trouble similar to that at Berlin, the admixture of water from another source is claimed to have brought about its extermination.

Dr. Zopf farther found that *Crenothrix* endured a temperature of 11 degrees Fahr., and when thawed out after several weeks' freeze, revived and developed new plants.*

In the United States *Crenothrix* has been found in moderate quantities in ground water supplies at several places in the State of Massachusetts,† and in larger quantities in the water supply of the city of Jamestown, N. Y.

Early in the summer of 1886‡ the water furnished by the Jamestown Water Supply Company became the source of such serious complaint that the Mayor of Jamestown addressed a communication to Governor Hill, requesting him, under authority of Section 8 of Chapter 322 of the Laws of 1880, to require the State Board of Health to investigate the difficulty. The Mayor's letter states "that the water which the Company furnishes is filled with dead and decayed fish, or portions of fish, decayed vegetable matter, and is thick and muddy, and has an offensive smell." The Water Company also wrote the Governor setting forth that they were the victims of "a long continued and malicious attack upon the quality of the water furnished through the

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*A more complete account of *Crenothrix* can be found in a paper, Natural Filtration, at Berlin, by Professor William Ripley Nichols, in Jour. Frank. Inst., Vol. CXIII (1882), pages 209 to 216. Professor Nichols has cited a number of references to the literature.

An abridged account can also be found in Professor Nichols' Water Supply, Chemical and Sanitary, pages 125 to 128.

† See Report of the biologist, G. H. Parker, in the Nineteenth Annual Report of the Massachusetts State Board of Health (1887), pages 89 to 94. *Crenothrix* is given as *Hypheothrix*.

‡ See Investigation of Jamestown Water Supply, in the Seventh Annual Report of the New York State Board of Health, 1886, pages 190 to 334. The various letters and reports referred to in the discussion may be found in the account of the proceedings in the Jamestown case there given.

mains by this Company," and heartily joining in the request for an investigation by the State Board of Health. The Company also recite in their letter that analyses have already been made by a number of eminent chemists (five in all), whose reports were at the service of any person who might be detailed for the investigation. On these requests the Governor referred the matter to the State Board of Health for investigation, which Board thereupon employed E. Kuichling, M. Am. Soc. C. E., for this purpose. The source of the water supply of Jamestown, as stated by him in his report, is as follows: First, from a series of driven wells from eighty to one hundred feet deep; Second, from the foot of Chautauqua lake, by means of a conduit, part wood and part iron, about two miles long. This conduit leads through a swamp and receives the water from the lake about two hundred feet from shore in a timber crib, with wire screens placed opposite the mouth of the conduit to prevent small fish getting in. The third source of supply is from the outlet of Chautauqua lake, near the pumping station. The original design of the Company was to obtain substantially its full supply from the driven wells, taking water from the Chautauqua outlet only on those rare occasions when the suppression of fires required the use of large amounts for a short time. As the use of water increased it was found that the yield of the wells was diminishing, so that another source of supply had to be secured, and with this in view the Company laid the conduit to the foot of the Chautauqua lake, as referred to above, but this conduit was not used until about the middle of September, 1886.

It appears from the reports that for two or three years previous to the formal complaint to the Governor in 1886, the Jamestown water had been a number of times the source of serious complaint on the part of the consumers, the trouble disappearing in winter and returning in spring and summer, and it was generally held by the citizens of Jamestown that the offensive tastes and odors were due to decomposing matter from the impure water of the Chautauqua outlet. The officers of the Company, however, denied that any considerable amount of water was pumped from the outlet, and maintained that only on the occasion of two large fires had it been used at all after June, 1885. In the meantime the most serious trouble had been experienced during the spring and early summer of 1886, becoming so serious as to lead to the request to the Governor by the Mayor on June 30th, 1886.

The investigation can be found at length in the place cited, and it is unnecessary to go into the detail here, except in a general way it may be said that very little definite light is thrown upon the subject. The chemical examinations show the water from the driven wells to be of excellent quality, as was also that drawn directly from the mains (see the chemical analyses in detail, pages 219 to 224, *loc. cit.*). None of the chemists report any bad tastes and odors, except Professor Leeds, in water taken from a dead end. Professor Leeds also attributes the trouble to algæ derived from the wells, but gives no intimation of the presence of *Crenothrix*. In the latter part of the month of December, 1886, after all the reports of the State Board of Health Investigation had been made, some samples of the Jamestown water were placed in my hands by Mr. Kuichling, with the request that I make an examination thereof. This was done with the result of easily making the identification of *Crenothrix*, and the photographs which accompany this paper (Plates IV and V) are from the samples furnished at the time. The matter was subsequently referred to Mr. Wolle, who verified the identification, and thereupon placed *Crenothrix* in his work on the Fresh Water Algæ of the United States, at that time just going to press.*

The suggestion that *Crenothrix* was possibly present in the Jamestown water was first made, so far as I am aware, by Mr. Kuichling. Owing, however, to lack of high power objectives, he was unable to verify his inference.† Nevertheless, considering the lack of information among water works engineers about cryptogamic botany, the making of the inference at all is an illustration of his broad attainments and may be justly cited as adding to his fame as an engineer.

In concluding this part of the paper, it remains to be stated that there is no absolute proof that the *Crenothrix* was actually derived from the driven wells. The company began to take water from its conduit to Chautauqua lake in September, 1886, while the samples containing *Crenothrix* only came to my hands in December, and the plant may have been derived from the swamp through which the wooden conduit passes from the pumping station to the lake. This wooden conduit is

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* See Wolle, *loc. cit.*, pages 294 of text, and Plate CCIX.

† *Crenothrix* can be seen, and, when one is familiar with it, readily recognized with a fair quality, one-half inch objective; but its complete study requires a good, recent, one-eighth, homogeneous immersion objective.

as I understand, loosely constructed, so that the possibility of such derivation probably exists. On the other hand, the persistency of the trouble when it had once appeared, and the fact of its only appearing after the material lowering of the ground water, would be a strong indication of its origin in the deep wells. Failure to find it at an early stage of the investigation can be accounted for by considering that no one was looking for *Crenothrix*. In any case, the whole matter is a good illustration of the importance of broad study in biology as an imperative requirement for success in such investigations.

Volvox globator, a unicellular form containing not only a mucilaginous jelly, but also starch grains in the chlorophyl (grass-green), and belonging to the order *Protococcoideæ*, is very widely distributed, being found in nearly all parts of the world. Its presence in water supplies has been noted,* but the fact that it will cause the fishy taste and odor has, I think, been generally overlooked. The water supply of the city of Rochester had, however, an infliction of this sort in the fall of 1888, the particulars of which are briefly as follows: In the early part of September, the Water Department began to receive complaints from various localities in the city that the water was permeated with an exceedingly strong fishy taste and odor, and the opinion was quite strongly held by many citizens that a large number of fish had in some way gained access to the mains, and that the difficulty was caused by their death and decay therein. It appeared, however, that the odor in question was precisely that of living fish rather than dead, and only a moderate amount of experiment is required to show that the odor from decaying fish bears no relation whatever to that which they exhale when alive. A daily study with the microscope showed that only the usual amount of animal and vegetable matter was present, with the single exception of an evidently sporadic development of *Volvox*. This alga was present in the distributing mains, and in both reservoirs† in vast quantities, the filtering of so small an amount as three hundred gallons showing many hundred thousands; while single dippings from the reservoirs without any con-

* See Report of the biologist, G. H. Parker, in Nineteenth Annual Report of the Massachusetts State Board of Health, *loc. cit*, where the presence of *Volvox* in a number of water supplies in Massachusetts is indicated.

† For detailed account of the relation of the storage and distributing reservoirs to Hemlock lake, the source of the supply, see the various reports of the Chief Engineer, especially those of 1872, 1874 and 1877.

centration by filtration showed the presence of the little globes in great abundance. This was the more remarkable from the fact that an almost daily study for nearly two years, and a frequent study for more than a year before that, had utterly failed to develop the slightest indication of *Volvox* in this water. So far as any record goes previous to last summer, *Volvox globator* had never been seen in the domestic water supply of the city of Rochester. No study of the matter was made at Hemlock lake itself until about October 20th, at which date only a few *Volvoces* were found, while at the same time both reservoirs teemed with them, and continued to so teem for two or three weeks thereafter. On the approach of cold weather the quantity gradually lessened, until in January it was only rarely that a single individual could be seen. In the meantime, the fishy taste and odor diminished as the *Volvox* lessened in quantity, and finally ceased to be perceptible to the senses as soon as the quantity of *Volvox* became small.

A satisfactory demonstration that *Volvox* was the real cause of the fishy taste and odor was made in the following manner.

Filterings from two or three hundred gallons of water were obtained by allowing the water to flow freely through a bag of plain, unbleached muslin attached to an ordinary kitchen cock. This concentrated everything contained in the quantity filtered into the volume of a few ounces. The concentrated filtrate was then passed through chemical filter paper with the result of leaving the contained *Volvox* behind on the paper, while clear water had flowed through into a clean Florence flask beneath. The *Volvox* was now washed off the filter paper with distilled water and the washing also introduced into a clean Florence flask. Gentle heat was then applied to both flasks, with the result of developing the fishy taste and odor with an intensity dependent upon the amount of *Volvox* present in the original filtering. After gentle boiling the contents of both flasks were emptied into open vessels and allowed to become entirely cold. On again pouring into the flasks and reheating, it was found that the contents of the first flask developed no farther fishy taste or odor, while the contents of the second flask again showed the taste and odor plainly perceptible. The significance of these tests is that in the case of the first sample, from which *Volvox* itself has been entirely removed by filtration through the filter paper, the application of heat the first time had driven off all the volatile matter causing the fishy taste and odor, so that after cooling nothing remained to be developed

by reheating; while in the case of the second sample, which still contained the original portion of *Volvox*, reheating again plainly developed the fishy taste and odor, but with less intensity than after the first heating. A repetition of these tests under varying conditions left no doubt as to the agency of *Volvox* in producing the fishy taste and odor.

The filterings contained in addition to living and decomposing *Volvox* a large amount of brown-colored amorphous matter, and that this was produced by the decomposition of *Volvox* was easily shown by allowing a concentrated filtering to stand for a short time, when, after the amorphous matter had settled to the bottom, it was easy to syphon off the supernatant fluid, containing only living *Volvox*. On heating the fluid so obtained and allowing it to stand for forty-eight hours, it was found on examination to contain an amorphous sediment precisely similar to that in the original filtering.

The considerable increase in the amount of animal life which followed this development of *Volvox* may also be noted as of interest. This was especially noticeable in the case of a number of the Rotifera, as, for instance, *Anuræa* and *Synchæta*. These transparent little creatures fed abundantly upon *Volvox*, and their structure and organs of assimilation are so clearly revealed under an ordinarily good objective as to exhibit the whole process of nutrition. The green-pointed spheres of *Volvox* could be easily seen rolling along through the œsophagus of the Rotifer on their way to the digestive cavity, where their gradual reduction to food and the ultimate rejection of the dross could be all clearly seen.

As to the reason for the sporadic development of *Volvox*, it appeared that some special cause had operated to produce a much greater development in both the reservoirs than in Hemlock lake, and a discussion of this point will lead us to consider briefly the salient points of reproduction and development.

Volvox may be described as a transparent sphere from one-fiftieth to one-eightieth of an inch in diameter, the inner surface of which is studded with groups of dark green points regularly disposed, and so arranged that each group of points is the center of a symmetrical group of six others. These green points are called gonidia, and each one is probably capable of becoming a perfect *Volvox*, though in fact only a small number of the many hundred with which every perfect specimen is endowed do actually so develop. If we consider, however, that under favorable conditions their growth is comparatively rapid, a ma-

ture individual developing from the embryo in a very few hours, and in its turn giving birth to a new colony, each of which sends forth its progeny, we are led to understand how a large body of water apparently entirely destitute of *Volvox* may in a few days be made to teem with it.

Extended observations of the alternations of the generations of *Volvox* show that its reproduction may take place in two ways, either sexual or a-sexual, and that the development from year to year is an alternation of these two methods, the usual course being development by one process in the spring, and by the other in the fall.* This alternation has for its climax the production of resting spores, which pass into a resting state, disappearing from view, and ultimately revivifying and producing the mature plant.

The *rationale* of the process of revivification is as yet unknown. Professor Cohn held that after passing into the resting state the spores must be desiccated before they could revive and produce living plants. This has, however, been contradicted by later observers; but the consideration that both of the reservoirs were entirely destitute of water during the summer of 1888 at any rate lends interest to this view in the present connection. Following this line it would appear that the extraordinary development in the reservoirs was from the revivification of spores which had possibly been dormant for several years, and which only developed after the absence of water from both reservoirs for the first time since their construction, had, by desiccation, produced the conditions necessary for revivification.

Moreover, the season of 1888 was one of unusually prolific development of *Volvox* throughout western New York, proven by finding large quantities in all the usual haunts. Hemlock lake, therefore, contained the moderate quantity which any such body of water would naturally contain in a season of unusually prolific development; while the reservoirs of the Rochester Water Works, by reason of having been entirely dry, presented the conditions for sporadic development. In any case the question is one of the greatest interest, but as this paper has already exceeded the limits originally set, it cannot be pursued at greater length here†.

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* See A. W. Willis on the Structure and Life-history of *Volvox*, in the Midland Naturalist, September, October, 1880. Also summary of same, in Cooke's British Fresh Water Algæ, pages 56 to 63.

† This account of *Volvox globator* at Rochester is abridged from two reports made at the request of the Executive Board by M. L. Mallory, J. Edward Line and the present writer, Members of the Microscopical Section of the Rochester Academy of Science. For these reports, giving a more complete account of the matter, see the forthcoming Annual Report of the Executive Board of the City of Rochester for the year ending April 1st, 1889.

It appears to the present writer that considerable advantage would accrue in the study of the important sanitary questions growing out of the relations of the algæ to public water supplies if we had a system of classification more in accordance with their real affinities in relation to the development of the troubles which we are now considering. Mr. A. W. Bennett has for another purpose proposed essentially such a system of classification, in which he places the colorless algæ in one class, those without distinct chlorophyl or starch grains in another, and those possessing pure chlorophyl and starch grains in a third. The use of this will assist by giving something on which we can systematically string our knowledge as it is slowly attained.*

A study of the kind here attempted would be incomplete without some indication of the real purpose of the algæ in the final economy of things. The late Professor William Ripley Nichols, who easily stood first as an accurate observer, remarked very justly that they cannot be regarded as an unmitigated evil; that they are purifying agents, changing at least a part of the dissolved organic matter into insoluble substanc which settle to the bottom.† This is a fair statement of the case, rendered more apparent by the consideration that nitrogen is an element largely needed in their growth, and that this is obtained from the nitrates and ammonia contained in the water. In adopting this view, however, we are forced to the conclusion, which is indeed sufficiently indicated in the foregoing, that the presence of certain algæ, in any given locality, may be a danger signal whose warning ought not to be lightly disregarded, while, in other cases, their presence is equally an indication that, impurities being present, nature is doing her very best to get rid of them. In any case, much remains to be done before the subject can be considered as strictly scientific in the sense of having a large stock of classified knowledge.

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* In Jour. Linn. Soc., Lond., Bot., XXIV (1887), pages 49 to 61. Also abst. in Jour. Roy. Micr. Soc., 1887, page 786.

† Sand Filtration at Berlin, Jour. Frank. Inst., Vol. CXII (1881), pages 436 to 440.

DESCRIPTION OF PLATES.

PLATE I (LXXVII).

Fig. 1 $\frac{20}{1}$. *Draparnaldia*, showing the penicillate, fasciculate branchlets. The broad chlorophyl band or main filament not shown.

Fig. 2 $\frac{100}{1}$. *Cladophora*, a series of articulate filaments, with the parietal cell contents shrunken, showing the thick, lamellose, gelatinous cell-membrane. (The shrunken matter is the chlorophyl mass containing the starch grains.)

PLATE II (LXXVIII).

Fig. 1 $\frac{20}{1}$. Two filaments of sterile *Zygnema* showing the usual form in which *Zygnema* will be found.

Fig. 2 $\frac{20}{1}$. *Spirogyra nitida* in conjugation. The filament on the left is sterile and represents the ordinary form. (This photograph is from a permanent mount and the chlorophyl bands in the sterile filament have shrunken by reason of the preservative medium used.)

Fig. 3 $\frac{20}{1}$. *Desmidium*, a single filament in the vegetative condition. The dark central portions are the chlorophyl masses of each cell.

PLATE III (LXXIX).

Figs. 1 to 4 $\frac{200}{1}$. A growing condition of *Ulothrix* which gave a strong pig-pen odor on decay. The chlorophyl masses, containing the starch (amylaceous) granule and the thin cell membrane, clearly apparent in all the figures.

PLATE IV (LXXX).

Crenothrix polyspora, Cohn.

Fig. 1 $\frac{500}{1}$. The central filament in process of development, with the exterior sheath clearly shown. The terminal members exceeding in length those towards the middle. (This is a typical development, according to Zopf.)

The curved filament at the right is another stage of development in the process of formation of gonidia.

Fig. 2 $\frac{200}{1}$. A typical mass of *Crenothrix* showing a number of fruiting filaments (sporangia). The irregular masses are points where gonidia have broken through the sheath and colonies of filaments are developing.

Fig. 3 $\frac{850}{1}$. The upper curved filament another typical form of development, according to Zopf.

Plate V (LXXXI).

Crenothrix polyspora, Cohn.

Fig. 1 $\frac{200}{1}$. An irregular mass of *Crenothrix* filaments, with structure obscured by a deposit of oxide of iron.

Fig. 2 $\frac{560}{1}$. A typical development similar to Plate IV, Fig. 1.

Fig. 3 $\frac{560}{1}$. A fruiting filament (sporangium), with fully developed gonidia. This filament shows the completion of the process faintly foreshadowed in the curved filament to right in Fig. 2, Plate IV.

Plate VI (LXXXII).

Fig. 1 $\frac{65}{1}$. *Hydrodictyon utriculatum.* A young net pressed flat and showing the reticulations of the saccate cœnobium.

Fig. 2 $\frac{65}{1}$. A number of reticulations in a more advanced state than in Fig. 1.

Plate VII (LXXXIII).

Fig. 1 $\frac{15}{1}$. *Hydrodictyon utriculatum.* A fully developed single reticulation, with the endochrome of the individual cells shrunken, showing the gelatinous exterior cell membrane.

Plate VIII (LXXXIV.)

Volvox globator.

Fig. 1 $\frac{100}{1}$. A fully developed parent cœnobium with two generations of young cœnobia within the parent. The first generation just ready to destroy the parent by breaking forth.

Fig. 2 $\frac{150}{1}$. Same as Fig. 1, but showing the peripheral cells, clearly, at the edge of the parent, and also on such of the first generation from the parent as are in focus. The beginning of the disintegration of the parent cœnobium is more clearly shown than in Fig. 1.

Fig. 3 $\frac{150}{1}$. A young cœnobium just after breaking forth from the parent.

Plate IX (LXXXV).

Volvox globator.

Figs. 1 and 2 $\frac{100}{1}$. Mature individuals illustrating stages of development somewhat different from any illustrated by the previous figures.

FIG. 1.

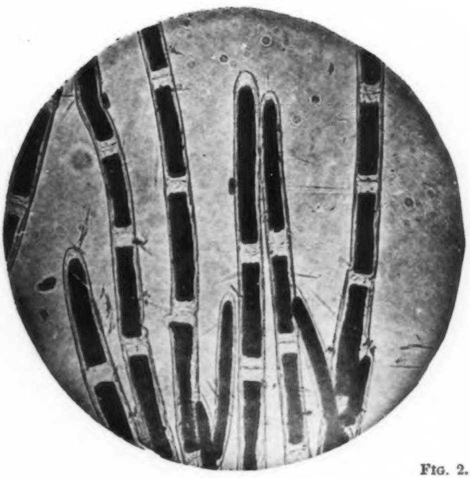


FIG. 2.

Photo-Micrograph,
by GEORGE W. RAFTER.

PLATE LXXVIII,
Vol. XXI, p. 504.

FIG. 1.

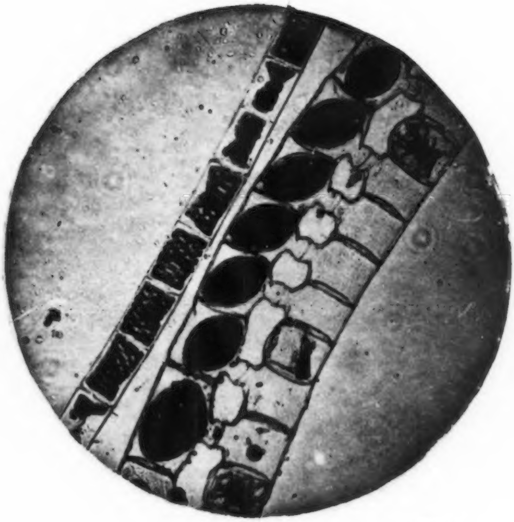


FIG. 2.

Photo-Micrograph,
by GEORGE W. RAFTER.

FIG 3

FIG. 1.

FIG. 2.

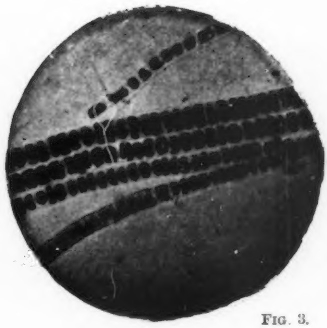


FIG. 3.

FIG. 4.

Photo-Micrograph,
by GEORGE W. RAFTER.

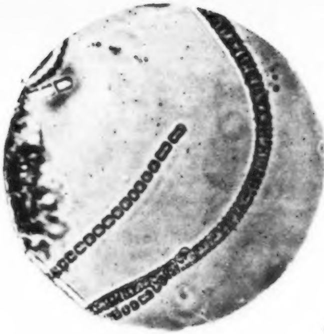


FIG. 1.

FIG. 2.

FIG. 3.

Photo-Micrograph,
by GEORGE W. RAFTER.

PLATE LXXXI,
Vol. XXI, p. 504.

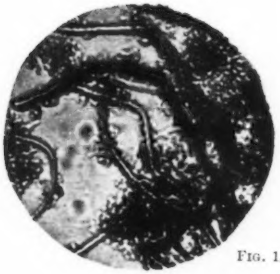


FIG. 1.

FIG. 2.

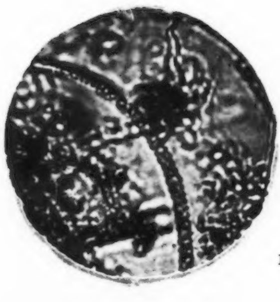


FIG. 3.

Photo-Micrograph,
by GEORGE W. RAFTER.

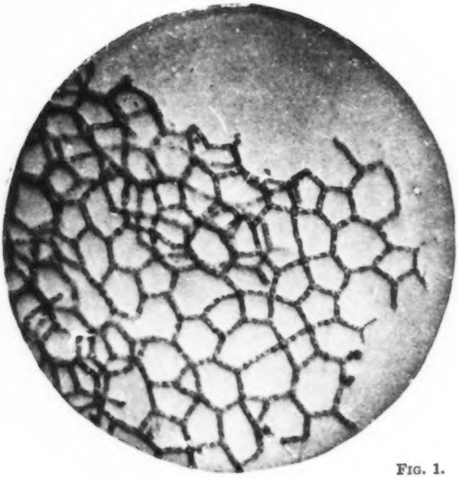


FIG. 1.

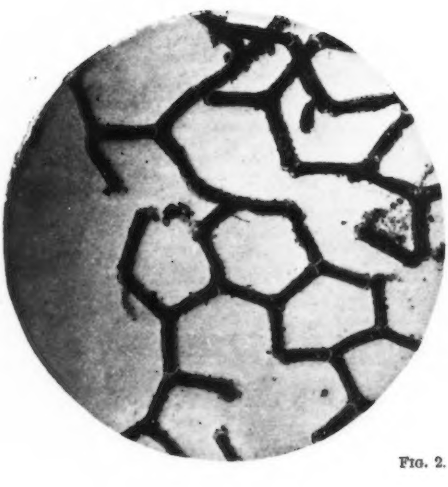


FIG. 2.

Photo-Micrograph,
by GEORGE W. RAFTER.

FIG. 1.

Photo-Micrograph,
by GEORGE W. RAFTER.

PLATE LXXXIV,
Vol. XXI, p. 504.

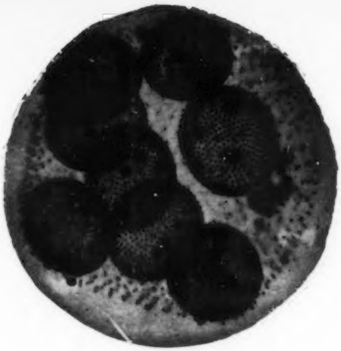


FIG. 1.

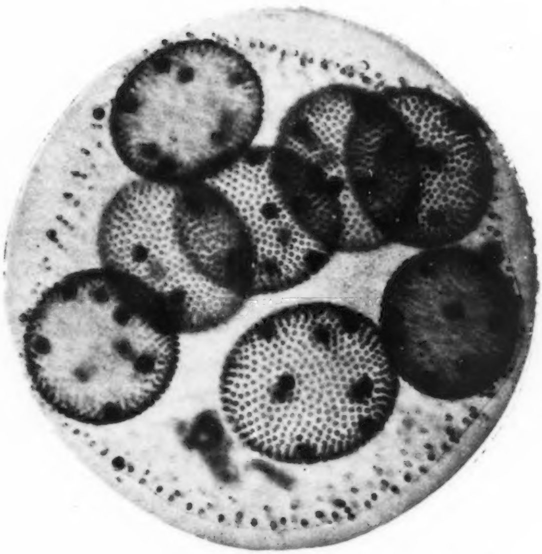


FIG. 2.

FIG. 3.

Photo-Micrograph,
by GEORGE W. RAFTER.

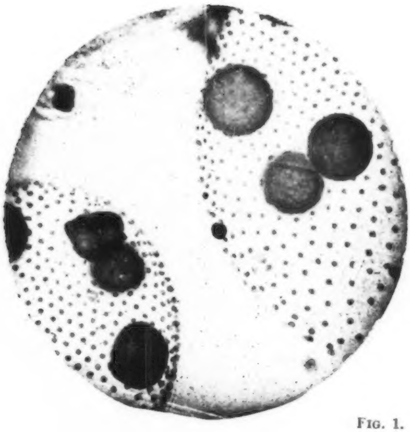


FIG. 1.

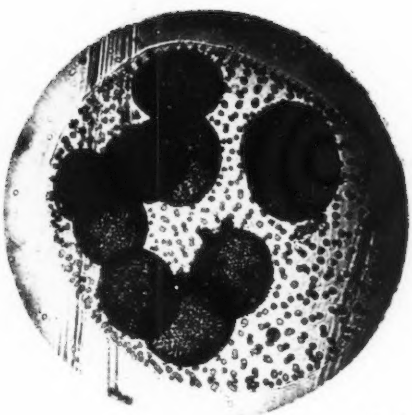


FIG. 2.

Photo-Micrograph,
by DR. J. EDWARD LINE.

DISCUSSION.

Professor ALBERT R. LEEDS.—I am hardly prepared at so short a notice to discuss the elaborate paper of Mr. Rafter in the manner which it deserves, and I can only bring to your notice a few observations in connection therewith, which I trust may be worthy of your attention.

In the first place I think it is of considerable interest, and especially to engineers of the City of New York, that one of the earliest inquiries into this subject was made by Dr. Torrey so long ago as the summer of 1859. At that season the water supply of the City of New York was extremely offensive. Dr. Torrey examined the water at Croton dam and found that much of it was of a bright green color, which color proved to be due to the accumulation of algæ. He took the water from the city hydrants and concentrated it by evaporation, when it developed a smell similar to that of the water taken at Croton dam. He took some of the water containing the algæ and distilled it. The distillate contained a volatile, oily material in which was concentrated a taste and odor similar to that affecting the whole water supply. He attributed this offensiveness to the growth of algæ in the water. He gave it as his opinion (one deserving of much attention as coming from so eminent a biologist, chemist and botanist) that while the water was so offensive, yet it was wholly without deleterious influence on health.

I notice that Mr. Rafter speaks of the development of the offensive taste and smell in connection with certain varieties of algæ containing starch. I note likewise that he states as the result of an experiment upon a solution of starch in water, that this solution after standing for several days gave a similar stench. In this connection I would suggest that the starch that he experimented upon was probably not chemically pure. The ordinary commercial starch is not, containing in addition to starch, which is a carbo-hydrate, some albuminous matter. The offensive taste and smell were probably associated with the albuminoid impurity.

Now in regard to the taste and odor affecting the water supply of the City of Jamestown. As consulting chemist of the Jamestown Water Company, I think it would not be right to allow this occasion to pass without protesting against what I think is an erroneous impression inadvertently conveyed by Mr. Rafter's paper. The Jamestown Water Company originally derived its water from a series of driven wells, sunk to a depth of 80 or 100 feet, believing that these wells were their purest obtainable source of water supply. To the best of my knowledge, which is derived from personal inquiries upon the spot, the company never resorted to the water taken from the outlet of Chautauqua lake except in case of fires. When this great trouble, this offensive taste and smell had been noticed, which was long prior to the use of the Chautauqua lake

water, they constructed a conduit to the foot of Chautauqua lake. They did not make use of the water from that source, however, until I urged them to do so. I made this recommendation because analysis had shown that the water taken from Chautauqua lake was of good quality, in fact not inferior in point of excellence (except in regard to its containing a higher percentage of carbon) to the water taken from the driven wells. I ascribed the presence of the algæ to the water from the driven wells, and connected with the growth of these algæ the presence of the large amount of oxide of iron in the mains. Unfortunately I did not make the identification of the algæ as *Crenothrix*. I am delighted to hear that that identification has been successfully made.

Mr. Rafter has quoted with approbation the monograph of Dr. Zopf, entitled "*Crenothrix*, the water calamity of Berlin." One could with equal truth say, "*Crenothrix*, the water calamity of the Jamestown Water Supply Company," because that company owed to the *Crenothrix* a persecution which for violence and injustice has rarely been exceeded.

A few words in regard to the development of diatoms. In the summer of 1886, the water in the Greenwood Cemetery reservoirs became extremely offensive, and Mr. Charles B. Brush, M. Am. Soc. C. E., submitted that water to me for examination. The supply was from driven wells. This water, filtered as it is through the surface strata of sand, in its original condition, is of great purity. But after being pumped up into the reservoirs it showed an abnormal development of diatoms. I attributed the bad odor and taste to the presence of these diatoms, or rather to the growth and decay of that gelatinous jelly which envelops them as a matrix. The analyses of the reservoir water as compared with those of the original water in the driven wells exhibited a great increase of carbonic acid in solution and of ammonia, and a great decrease of dissolved oxygen.

The chemical phenomena in the Greenwood Cemetery reservoir were similar in their character to those in the water of the Hoboken reservoir in the summer of 1884, although in the latter case the growths were not diatomaceous, but were due to the development of algæ belonging to the genus *Oscillaria*.

I have omitted saying anything as to the suggested remedy. When the Jamestown Water Company requested a remedy, I suggested two—either to exclude light from their reservoir or to aerate the water. They elected the former and covered the reservoir. The exclusion of light had not the slightest influence in preventing the growth of the algæ and the development of the offensive taste and smell.

Professor Elwyn Waller.—It is certainly an encouraging sign of the times that the growing interest in sanitary science has induced, among other things, a more careful study of the phenomena connected with odors in water supplies during the last fifteen years or so. I remember that in 1876, when, as chemist to the New York Board of Health, I was

called upon to investigate the subject in connection with the appearance of odors in the Croton water, the record of observations on the subject was exceedingly meager and unsatisfactory. Our knowledge, however, is not yet in such a form that in any given case we can immediately detect the cause and apply a remedy, but a considerable advance has been made.

In the investigation referred to I found that out of say one hundred and fifty public water supplies in various sections of the country, about half of them had had some experience of the kind. Of those places where, within the knowledge or recollection of those then having charge of the water supply, no experience of odors in the supply had been had, several have since had manifestations of the phenomena. In the replies which I received to a circular-letter of inquiry the data furnished were often insufficient, especially where no odor was reported; but, so far as I could learn, of those reporting no odor, nine or ten at least were from artificial reservoirs, and four were on limestone formations. Of those where odors had appeared, at least twenty-six drew their supplies from artificial and eight from natural lakes. Naturally fuller data were given where odors had been perceived, and it may be erroneous to generalize from these figures.

When the matter first attracted general attention, and the algæ were said to be the cause, many doubts were thrown on this view of the case. I remember, in 1876 or 1877, receiving a letter from Professor W. R. Nichols on the subject, in which he remarked, "There is too much of the *Post hoc, ergo propter hoc*" about this. In an annoying case of the occurrence of the "cucumber odor" in the "Bradlee basin" at Boston in 1876, report was made by Professors Nichols, Farlow and Burgess that the water contained apparently no more chemical substances or vegetable or animal life than usual. Later on, with a recurrence of the trouble, possibly from the same source, Professor Remsen found the cause in the decomposition of the *Spongilla fluviatilis*, which, having been attached to the sides of the reservoir and not floating in the water, may have perhaps escaped notice in 1876.*

The fishy character of the odor in some cases led many to suspect that the presence of considerable numbers of fish, or the prevalence of some disease among them was the cause. At several places reservoirs were emptied at considerable expense, only to find comparatively few fish, and those few in a healthy condition. The thought has occurred to me that in some cases more fish instead of fewer might be a remedy,

* Note by G. W. R.—The sponge which Professor Remsen found at Farm pond, and figured in his Report on a Peculiar Condition of the Water at Boston, in November, 1881, is properly, by the recent work of Mr. Potts, *Meyenia fluviatilis* instead of *Spongilla fluviatilis*, Auct, as given by Professor Remsen on the authority of Mr. Hyatt. For modern classification of fresh water sponges see Contributions towards a Synopsis of the American Forms of Fresh Water Sponges, etc., by Edward Potts, in the Proceedings of the Academy of Natural Sciences of Philadelphia, April-August, 1887.

by serving to preserve the proper balance between the animal and vegetable forms of life. In some cases, however, a fungoid growth on the bodies of the fish has accompanied the presence of odors in a water (Haverhill, Mass.). Professor Nichols, in his book on Water Supply, suggests that when the algæ form scums on the surface of the water it may cut off the supply of air necessary for the fish.

The theory of the production of innumerable bacteria, when there is nothing for them to feed upon, seems unlikely. Other theories were advanced, some more or less absurd. I recall one, that the action of the water upon vegetable matter produced crenic and apocrenic acids which attacked the iron pipes, and thus gave off those ill-smelling compounds which result from the solution of cast-iron in an acid. This is untenable, since the odors are not the same, and moreover frequently have appeared where the water is not and has not been in contact with iron pipes.

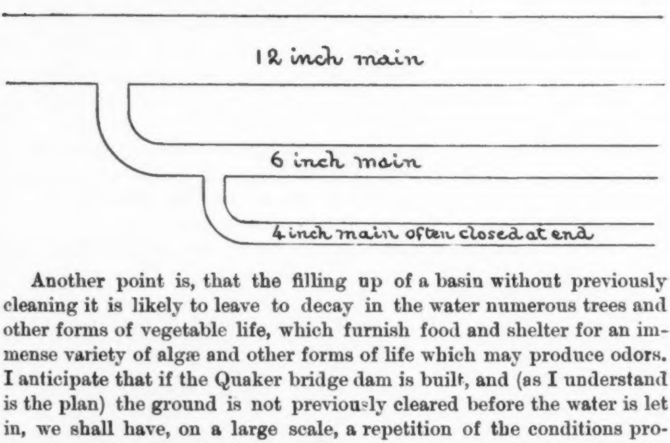
It seems now that we can accept the view that frequently, though not necessarily always, the odors in water supplies are attributable to algæ, or some other vegetable growth. Many direct experiments, such as those quoted by Mr. Rafter as having been made with the *Volvox*, constitute strong arguments in this direction. In general the presence of large quantities of certain forms of algæ just previous to or during the prevalence of odors is another. As to the counter-argument that the odors have appeared sometimes in cold weather, when the conditions are apparently less favorable for vegetable development, we seem to have found a reply in the point made by Mr. Rafter, that the algæ are extremely hardy, and not only survive but may flourish at comparatively low temperatures. With larger plants some will grow during winter, though the warmth of summer favors the most of them, and as to the algæ as a class, some representatives flourish on the snow, while others again have their habitat in hot springs. As to odors in water, they occur most frequently in summer, though they sometimes appear in winter.

It seems probable that a fair uniformity of temperature has a considerable influence; that the algæ observed by Mr. Rafter in March, when the snow had scarcely gone, would have been destroyed by a sudden rise in temperature, though they might, perhaps, endure and flourish if the temperature were gradually raised to that of summer. A sudden fall of temperature was noted to be fatal to certain of the *Nostocs* (Albany), and a high temperature is recorded as having killed multitudes of the *Clathrocystis* (Horn Pond, Mass.).

In this connection it may be well to recall the observation of Mr. T. W. Davis, at Poughkeepsie, that when the temperature of the water in the filter beds passed 70 degrees Fahr., the odors in the water began to cause complaint. From this we may only legitimately conclude that the temperature of 70 degrees and over will favor the development of odors from certain algæ.

This serves to emphasize one point of importance, that reservoirs for water should not be too shallow.

Some other points which Mr. Rafter did not mention may be here alluded to as apparently having influence on the development of the offending algæ. One, that in some cases the establishment of a circulation of the water in the reservoir has mitigated or removed the trouble (New Haven). The effect may have been due to aeration, to prevention of stagnation, or to both. In that connection it may be said that the testimony as to the prevalence of the odor in dead ends of a supply system is very conflicting. Observers as to this point may sometimes be deceived, at least in some of our up-town streets in New York, where, as I am told, in many cases, though a large main in which there is plenty of flow runs through the center of the streets, the houses on the side of the streets draw from a second or third side main parallel to the chief one, and are in reality drawing from dead ends. The arrangement as described to me is something like this:



Another point is, that the filling up of a basin without previously cleaning it is likely to leave to decay in the water numerous trees and other forms of vegetable life, which furnish food and shelter for an immense variety of algæ and other forms of life which may produce odors. I anticipate that if the Quaker bridge dam is built, and (as I understand is the plan) the ground is not previously cleared before the water is let in, we shall have, on a large scale, a repetition of the conditions produced at New Britain, Conn. There the town could not afford the expense of cleaning the reservoir site, and they had an appearance of odor every summer, which was less and less every year for about twelve years. In 1874, the thirteenth year, I believe, they had a short and sharp attack, lasting about two weeks, during which time the President said it had been "never worse."

Among the conditions favoring the development of algæ, or upon which some of them are dependent, Mr. Rafter mentions the presence of certain mineral compounds, as sulphates for *Beggiatoa*, etc.; lime for *Batrachospermum*, etc.; iron for *Crenothrix*; silica for *Inactis;* and sodium chloride for marine forms, and possibly in smaller amounts for

most fresh-water forms as well. If he could add to the list potassium and magnesium compounds, we would have the full quota of mineral substances which are usually found in all natural waters. These observations are of great interest, but will be of limited service in measures of prevention or cure.

As to the more immediate cause of the odors, Mr. Rafter states that they may be caused (1) by volatile oils; (2) by decaying starch; or (3) by decaying nitrogenous jelly secreted by certain forms. The latter, in his estimation, is probably the usual one. This brings up the rather important question as to whether the odoriferous principle is developed during the life or after the death of the algæ. From the evidence which has been collected, it seems reasonable to conclude that in some cases it is the one; in other cases, the other. It seems probable, or at least possible, that the formation of a volatile oil may be coincident with the fructification of some algæ, and that this is immediately followed by the death of the parent, and under such conditions it would be a difficult matter to decide the point.

The presence of any volatile oil in *Nostocs* and many other odor-producing algæ is denied by Mr. Rafter. In that connection I may mention the extraction of a fatty substance, soluble in ether, called by Dr. Torrey "vegetable wax," from the residues of water teeming with *Nostoc*. It was without apparent odor.

The observation as to the nitrogenous character of the jelly was illustrated by an examination which I had occasion to make on the Croton water in 1881, probably at the time referred to by Mr. Rafter as mentioned by Mr. Hyatt.

Of the worst samples the filtered water contained but little ammonia, chiefly albuminoid, but the unfiltered water showed considerable amounts of both kinds of ammonia.

Mr. Rafter further remarks upon certain forms of life which have hitherto been regarded as innocent of the production of odors, and makes out a strong case against *Volvox* and some diatoms. The remark as to the appearance of a rotifer as the enemy of *Volvox* suggests a similar observation of Dr. C. T. Jackson, in connection with the Cochituate water, that *Cyclops* was an enemy of certain odor-causing substances or organisms. Moreover, Professor Lupton reported upon the examination of the water of Nashville, Tenn., when it was affected, that *Cyclops* was very abundant, and in one case of the trouble in the water supply of Trenton, N. J., Dr. Henry Wurz attributed it to the presence of *Cyclops*. We have here either another supposed innocent organism to be added to the list, or a suggestion of a possible destroyer of certain objectionable forms of vegetable life, at least, objectionable when appearing in such overwhelming numbers.

In the light of these facts may it not be the case that any one of the lower forms of life dwelling in fresh water may at times develop to an

abnormal extent, and cause odors in water by the death and decay of myriads of individuals, which are destroyed in the struggle for existence with one another? Taking that view, it is useless to designate one as an odor-producing species and another as innocent, since any one form, whether animalcule or alga, may be the cause in any particular case, and the question would bring itself down to the proper preservation of the balance between the various forms of life. Our superintendents of water works would then have to be students of aquaria.

It is no doubt true that the superintendent who regards the reservoir under his charge more in the light of an aquarium than of an exaggerated water tank, will be the more successful in his management.

Finally, Mr. Rafter suggests that the presence of these odoriferous algæ may be a danger signal. As to that, we hardly know enough on the subject to venture upon even a very guarded statement. Certainly so far as the evidence goes, there is no particular danger to health in swallowing considerable numbers of various species of algæ which directly or indirectly cause the odor. So far as I know, but one case of a poisonous alga has been put on record. *

This was in Lake Alexandria, Australia. The plant was said to be a *Nodularia*, and developed vigorously when the temperature of the water was 74 degrees Fahr. Domestic animals which drank the water containing it were seized first with stupor, then with spasms, which were followed by convulsions and death. It no doubt would also have been fatal to human beings.†

As to preventive methods which have been found efficient in some cases, I have already alluded to the attention to the question of temperature, of circulation, and also the removal of vegetable débris from the reservoir site. Another suggestion may be found in the experience of Mr. McFadden at Philadelphia, where the presence of odors was prevented by the simple expedient of taking the supply from below the surface. Any one or all of these means may, and indeed have, frequently proved ineffectual. We evidently need much more information on the subject, and consequently such information as has been contributed by Mr. Rafter is most welcome.

One of Mr. Rafter's closing remarks, though not perhaps based on a misconception, might prove misleading. He seems to imply that algæ may be purifying agents because nitrites and ammonia form a part of their sustenance. To this I feel constrained to remark that the nitrites and ammonia, even in the most contaminated waters, are absolutely

* Nature, XVIII (1878), page 11.

† Note by G. W. R.—A similar case of a poisonous alga is reported from Minnesota. J. C. Arthur has given an account of it in the "Bulletin" of the Minnesota Academy of Natural Sciences, Vol. II, Bul. IV, and Vol. III, Bul. I. The investigation, however, was only made after the fact, and some doubts remain as to the alga being the cause of the trouble. Mr. Arthur claims in his report to have finally identified the offending form as *Glœotrichia Pisum*.

harmless in the proportions in which they there exist, but that their presence in comparatively large proportion is an indication of the presence of nitrogenous organic matter in a state of decomposition which is probably therefore dangerous. If, then, the nitrites and ammonia are removed from a water, it may still be dangerous, but in the present state of our knowledge, a chemist would be likely to pronounce it safe.

A. Fteley, M. Am. Soc. C. E.—Mr. President, my excuse for rising to discuss this subject is that, having been engaged in a number of works where a great deal of annoyance was caused by the presence of algæ, I have been obliged, out of necessity, to study the question as much as it was in my power to do.

Those who have had occasion to study questions of drainage and water supply, involving the welfare of large communities, will agree with Professor Leeds that their solution is very often embarrassed and retarded by the introduction of subjects entirely foreign to the scientific character of the matter in hand, based mostly on local prejudices, and consequently introducing in the debate an amount of bitterness which is very detrimental, if not absolutely fatal, to the proper consideration of technical points.

To that cause, I believe, must be attributed the doubtful result of investigations which, if not interfered with, would have probably increased our field of knowledge.

As an illustration of the remarks just made, I remember that during one of the periods of "bad water" in Boston, at a time when excitement ran high on that account, outsiders, without sufficient knowledge of the resources and local conditions of the water supply, aggravated matters by publishing descriptions and analyses of water collected at places partially disconnected from the sources of supply and supplied in small quantities at great distances from the point of taking. The facts were most probably true, but, without proper explanation, they were misleading.

There is, most probably, a great diversity of causes for the bad tastes and odors in public water supplies; they may be often traced to the presence of microscopic vegetable growths in the reservoirs where the water is kept stored in large volume, or at any rate these growths are contemporaneous with them; but, at times, the same trouble may occur without their presence. Such has been my experience in regard to three reservoirs containing from 200 to 1 100 million gallons, which I closely observed for several seasons.

It is possible that my want of skill as a microscopist may have prevented me from finding in the water spores of algæ or other minute bodies which might have been the cause of the trouble, but at times I have failed to find, with a glass of high power, any algæ or other microscopic bodies, while at other times I could readily determine the various species which were generally found in those waters.

Professor Waller has referred to Professor Nichol's experience in regard to Bradlee basin, in Boston. As the occurrence is familiar to me, I may be permitted to add a few details which apparently confirm the statement just made.

Bradlee basin is one of two twin reservoirs from which the water received from the storage grounds of the system is mainly distributed into the city; they are separated from one another by an artificial embankment—one, the Bradlee basin, having been excavated to the rock bottom, while the bottom of the other is formed by the original meadow soil; but the water in the Bradlee basin, with a rock bottom, had such a disagreeable taste that its use had to be discontinued, while in the other division with the meadow bottom it was of sufficiently good quality to be distributed to the consumers. A few weeks after the shutting off of the contaminated basin the taste disappeared, without apparent cause, and the water was again used without complaint.

The irregularity which seems to accompany the symptoms just described is found also in the distribution of the taste among the various places of consumption. At times the bad taste seemed to be limited to certain parts of the city; at other times, although general, it was not of equal intensity in different places, or it even disappeared at irregular intervals. A singular fact, reported several times to me, was that while the taste was very pronounced on the ground floor of the house, it would be almost imperceptible at the top.

As to the habits and appearance of algæ in the reservoirs that I happened to examine with some care, I may be permitted to refer to a description published in one of the reports of the Massachusetts Board of Health; the facts therein noted may be found of interest:

"The causes of the alterations observed, especially during the summer, in the quality of the water of our public supplies, remain very often unknown, and local observations are generally found to be insufficient. A complete system of observations extending over several years, covering an extensive territory, and embracing various descriptions of water supplies, would be needed to throw any light on the question.

"The following observations have been limited to some of the ponds connected with the water supply of Boston. They cover too short a time to be conclusive, but they have furnished a few facts which may suggest some new and more complete researches.

"Of the three sources of water supplies of Boston, one, the Cochituate, has presented no unusual feature this year; the new supply from Sudbury river was in a peculiar condition. Although its water has been furnished in a large proportion to the city for the last five years, the new reservoirs were filled this year for the first time. Notwithstanding these unfavorable conditions, the water has been unobjectionable in quality; but it has been observed that sulphureted hydrogen was found in the reservoirs during July and August, and could be plainly per-

ceived when the water was flowing through the sluice-gates. The smell of the gas disappeared after a short exposure of the water to the air, and none of it was ever detected in the conduit.

"The temperature of the water in the reservoirs at the time that the presence of sulphureted hydrogen was detected in it was remarkably high, as shown by the accompanying diagram; and it was identical at top and bottom, although the depth at the point of observation was more than 20 feet. A number of readings taken in June, July and August, and not noted at the time, showed generally a similar result, which may suggest the idea that the temperature of the water was kept uniformly high by chemical action. The disengagement of sulphureted hydrogen ceased apparently toward the end of August; but it may be seen by the diagram that, since that time, the temperatures at top and bottom of Reservoir No. 3 have continued to be singularly near one another.

"The principal trouble experienced this summer about the water supply of Boston was due to the presence of an abundant formation of algæ in some of the ponds of the Mystic supply, and also, to a less extent, in one of the new reservoirs on Sudbury river (Basin No. 3).

"These minute plants, which appear to be uniformly distributed throughout the water, flow with it, and are of such a small bulk that they cannot be separated by screens. The wind has a noticeable effect on them, and often blows them toward the lee shore, where they accumulate and form a solid scum of a sharp green color. When in the fresh state they emit a peculiar musty odor; if stranded by the action of the wind, they soon decay and form a bluish-green mass which develops a smell as of organic matter in process of decomposition; when in the water the algæ remain suspended for some time, and, after a while, sink to the bottom. This result, which it would be difficult to observe directly in a large mass of water, is indicated by the fact that the large quantities of algæ which have been found in the reservoir, at a time when there was no outflow from it, have diminished several times, and ultimately disappeared, without leaving at the surface or on the shores any indication of their presence. The same result has been obtained several times by putting in a closed bottle water containing some of the green scum collected in Reservoir No. 3; after a short time, one or two days, a portion of the green matter, of a light shade, sinks to the bottom; examined under the microscope it proves to be composed of one kind only of algæ, presenting the appearance of strings of single cells; the rest, of a darker green, containing plants of a rounded shape, floats on the surface for a few days, and then sinks; before the end of one week all the vegetable matter is at the bottom, where its color changes from day to day from green to brown. Almost from the beginning of the experiment, the contents of the bottle emit an extremely offensive odor of decomposition, which increases as the plants decay and lose gradually their characteristic form."

Of the formation of the algæ, or of its cause, little is known, but it is remarkable that they appear suddenly in large quantities. Shallow flowage, it is said, favors their development, probably on account of the higher temperature which the water attains in such conditions when heated by the sun; but they are formed very rapidly also in deep water. I have observed several times that large quantities appeared in a very short time, equally distributed through hundreds of millions of gallons of water 20 feet deep, several hundred feet from any shore, and in very calm weather; the quantity of algæ in the water, as observed in Basin No. 3, seemed to vary constantly, increasing or diminishing with wonderful rapidity.

I have some reason to believe, from the statements of various persons, that the same kind of vegetation has been observed in years past in this vicinity; but in the absence of reliable evidence on the subject, some doubt may exist about the identity of the vegetable formations.

It has been suggested that the development of the algæ in the Mystic supply may have been favored, if not caused, by the presence of the sewage matter which finds its way into it; but the algæ have been observed also, in less quantity, however, in ponds which are entirely free from such pollutions. It is particularly remarkable that no vegetable growth of the same kind has been seen in the Abajonna river or its mill ponds, although they receive some very objectionable drainage, at the time that the other branch of the Mystic supply, the Horn pond branch, has been full of it. This fact is so much more noticeable, because Horn pond, where the algæ have been found in great abundance, is very deep generally, and presents a small proportion of shallow flowage. A somewhat similar condition of things has been observed in Framingham, where algæ have been growing in Basin No. 3, a deep, artificial reservoir built on the Stony Brook branch of Sudbury river, while Basin No. 2, on the other branch of the river, and of less depth, has been entirely free from vegetable growth.

The accompanying diagram, Plate LXXXVI, shows simultaneous observations made in October, November and December. It does not seem that the degree of the humidity of the atmosphere, or the barometric pressure, has any marked effect on the algæ; but their formation appears to follow the temperature, especially the temperature of the water, increasing and diminishing with it. The scale for the diagram showing the amount of algæ found in the water indicates only proportional quantities obtained by daily observations. To determine these quantities the water was examined at certain given stations, each representing a portion of the total area; and samples procured at each station, at top, middle and bottom of the water, were compared to a constant standard. Coefficients proportional to the areas represented by each station, and to the degree of turbidity of each sample as appreciated by the eye, were then used to establish the daily returns. These results may not be

strictly correct, but it is believed that they represent very fairly the condition of the water.

Although screens fail to free the water from the algæ, the latter can be retained on filter-beds constructed in the usual manner; when they are abundant, however, they form in a short time an almost impervious covering over the filtering surfaces. To filter large quantities of water under these conditions would require very extensive filtering areas and frequent cleanings, at a great expense.

A small experimental filter, one yard square, made of gravel and sand, and constructed in the well-known manner recommended for water supplies, showed that, in operating on water containing a moderate quantity of algæ, the filtering capacity of the apparatus was reduced in eight days from sixty gallons per twenty-four hours and per square foot to seventeen gallons; another time it was reduced in the same number of days from seventy-five to fourteen gallons.

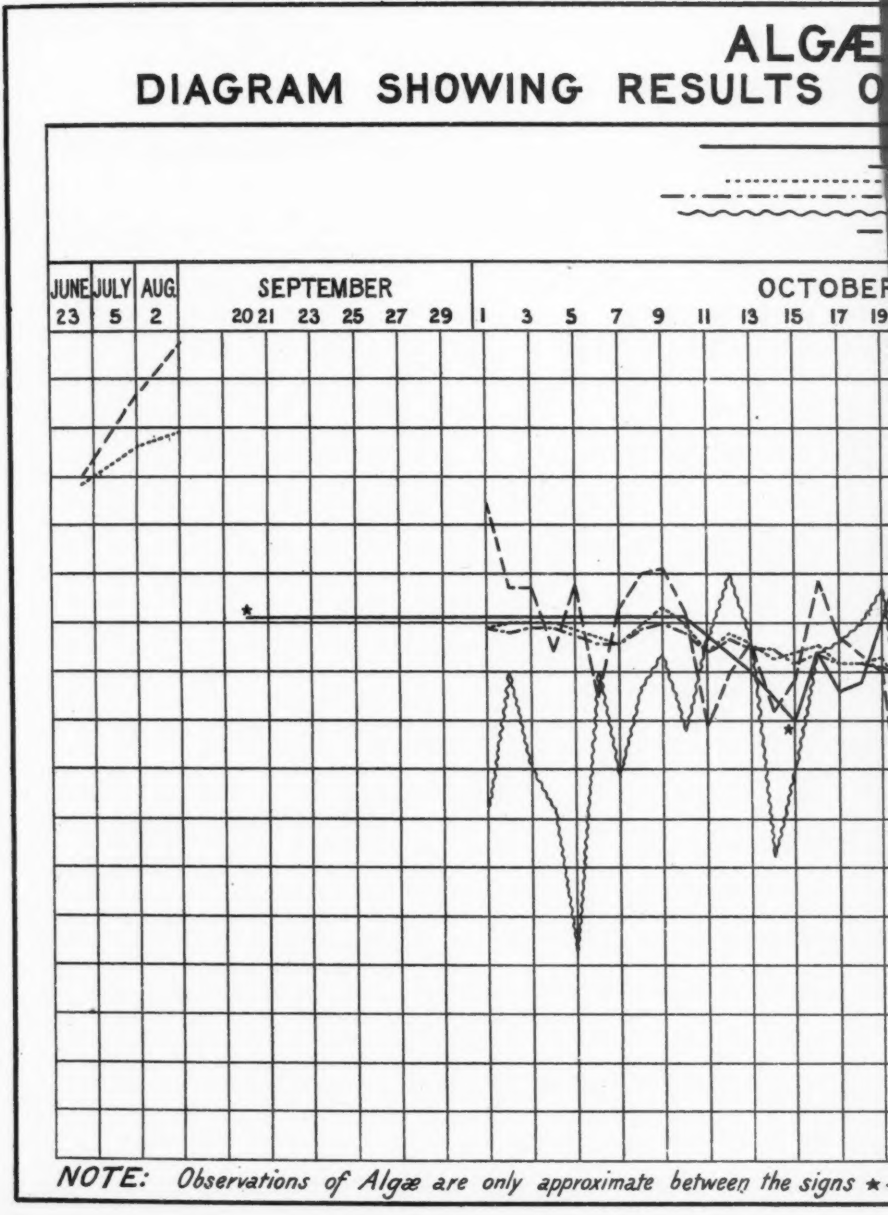
The sand used being rather too fine, the filter was made over again, with well-washed sand, from which all fine particles had been excluded by using a wire sieve of twenty-four meshes to the inch; in this case the filtering capacity was reduced in eleven days from one hundred and nine to twenty-three gallons per square foot and per twenty-four hours. The table below shows the daily results of the experiments.

NUMBER OF GALLONS FILTERED PER SQUARE FOOT AND PER TWENTY-FOUR HOURS.

	First experiment. Gallons.	Second experiment. Gallons.	Third experiment. Gallons.
First day	60	75.1	109
Second day	50	19.2	109
Third day	53.3	18.5	120
Fourth day	40	17	80
Fifth day	19.17	16	80
Sixth day	18.50	15.5	65
Seventh day	16	15.5	54
Eighth day	17	14.1	42.8
Ninth day	....	....	34.3
Tenth day	....	....	26.6
Eleventh day	....	....	22.8

At the rate of the last experiment, it would require 233 000 square feet of filtering surface to filter ten million gallons per day, and each filter bed would need cleaning once a week.

After each experiment there was found on the surface of the filter a slimy, greenish matter, presenting the peculiar musty odor of the algæ; and the surface, when dry, became so much hardened by its mixture with the sediment, that, to the touch, it closely resembled slightly frozen sand. The sand did not appear to be penetrated by the vege-



ALGÆ
DIAGRAM SHOWING RESULTS O
JUNE 23
JULY 5
AUG. 2
SEPTEMBER
20 21 23 25 27 29
OCTOBER
1 3 5 7 9 11 13 15 17 19
NOTE: Observations of Algæ are only approximate between the signs ★

LGÆ IN STORAGE RESERVO

LTS OF OBSERVATIONS IN BASIN 3,

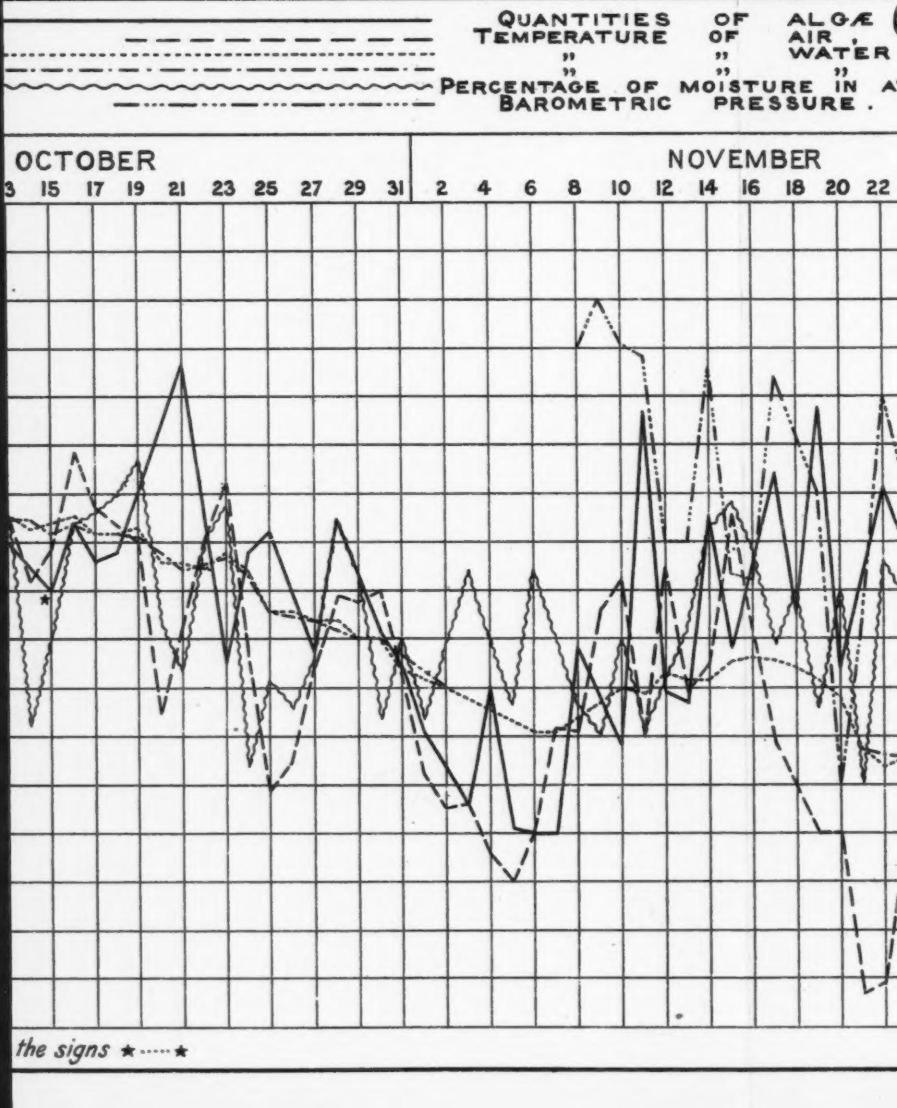


PLATE LXXXVI
TRANS. AM. SOC. CIV. ENG'RS.
VOL. XXI. Nº 427.
FTELEY ON PURITY OF WATER

RVOIRS.
3, FRAMINGHAM, MASS. IN 1878.

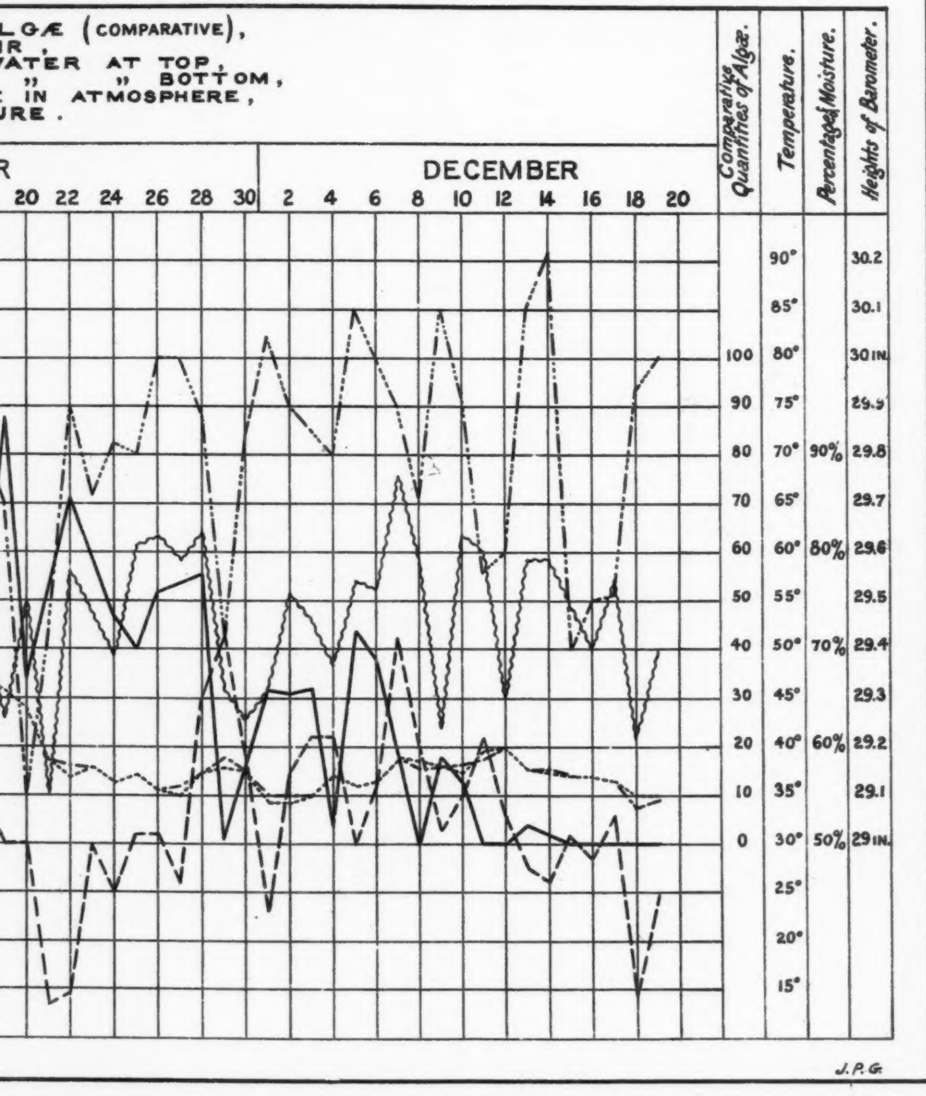


table deposit; a slight scraping of its surface showed it very free from foreign matter.

Professor Leeds referred also to Professor Nichol's examination of Horn pond in Woburn, Mass., in 1873 or 1874. It that case the appearance of the pond was very different from what I have seen elsewhere; the stench arising from the water, the characteristic "pig-pen" odor was such as to drive away some of the inhabitants from the shore, and the surface was covered with a thick, slimy deposit, looking like animal matter in decomposition. This idea was encouraged by the fact that several tanneries were then draining into the pond; but the surface slime was recognized as decomposed algæ, and after a short time it disappeared, leaving the water in its ordinary condition.

The origin of the bad tastes and odors in public water supplies is, at best, obscure; and in many cases it is probable that they have been admitted as due to causes which were not sufficiently studied.

In 1881 the water in Farm pond, near Boston, became exceptionally bad in taste; the trouble became so serious, owing to the fact that all the supply from Sudbury river had to pass through that sheet of water, that the pond was emptied, and in a few days an artificial ditch, over 4 500 feet long, was constructed, partly in the sandy bottom of the pond, partly in a timber flume supported by sheet piling, of sufficient capacity to convey independently from the pond a maximum of forty million gallons. Professor Remsen, of Baltimore, was called to investigate the matter, and much credit is due him for having discovered in the waters of the pond a species of sponge, the presence of which had not before been suspected in the water supply. This *Spongilla* was declared to be the cause of all the trouble. Without being able to refute absolutely that opinion, I believe that the conclusion arrived at is too sweeping in its character. Farm pond is about 200 acres in area, and, at that time, the water contained in it was constantly changing to some extent, owing to the fact that a large proportion of the city supply was flowing through it; when it was emptied, the *Spongilla* was collected from a large proportion of the bottom which became exposed, but the whole of it would not have filled a bushel basket. It is difficult to suppose that this small quantity could have contaminated to such an extent so large a volume of water. Moreover, after collecting a bucket full of the *Spongilla*, I had it washed several times in well water, and although it was allowed to remain in that water for a considerable time, the bad taste was not reproduced. The same *Spongilla*, when again immersed in pond water, very soon resumed the old taste to such an intensity as to suggest a remarkable power of absorption.

As I am on the subject of Farm pond, I desire to state that when the construction of a permanent aqueduct across it was first spoken of, J. P. Davis, M. Am. Soc. C. E., then City Engineer of Boston, suggested the idea of adding to it an apparatus for forcing air through water. The

suggestion was not carried out, but may properly be mentioned in connection with the remarks on the same subject by previous speakers.

In the course of the discussion on Mr. Rafter's paper, a distinction has been made between natura and artificial reservoirs of water. It is possible that the necessities of a water supply, by producing rapid changes in the volume of water contained, and excessive fluctuations of the water mark, may place artificial reservoirs in exceptional conditions, favorable to the development of some of the evils complained of, but it is most probable that the differences between the two classes of sources of supply are sometimes imaginary, from the fact that artificial reservoirs are more closely observed, and that the slightest deterioration of their waters is at once necessarily discovered. I recall hearing loud complaints from people living in the vicinity of Farm pond (already mentioned) as to the deterioration of its water since it had become a part of the water supply of Boston; while an investigation disclosed the facts, well known to the oldest inhabitants, that the surface was, previously, sometimes covered with patches of green stuff which were then supposed to come from an accumulation of the pollen fallen from pine trees, and that the water was sometimes in such a condition of "fomentation" that when going fishing the thirst was better quenched with something stronger than the bad tasting pond water.

An allusion has been made in the course of these remarks to the possible trouble to result from the growth of algæ in the very extensive reservoir contemplated by the City of New York in connection with its new aqueduct in the valley of the Croton river. The opposition to the building of that reservoir was so great from some quarters that the Aqueduct Commissioners requested the Board of Health to give them its views in regard to the probable sanitary condition of the proposed reservoir. The board and its advisers investigated the question with care, and reported that the great depth of the lake would be conducive to the purity of the water. I fail to see that the chances for the formation of algæ will be more numerous in the future than at present, and this growth depends so much on local conditions that a comparison with the present reservoir is probably the safest ground on which to base an opinion. The present Croton reservoir is a comparatively small, shallow sheet of water, subject to numerous fluctuations which expose during the summer large areas of land, thus producing conditions favorable to the growth and development of algæ ; its shores present at times a very objectionable appearance when the water is low; and yet for more than forty years it has furnished to New York a very acceptable supply superior to that of some other cities. The advantage with the same local conditions is in favor of the larger and deeper reservoir.

As to the advisability of removing the loam and all perishable matters from its area before construction, it is clearly out of the range of practicability, although some opponents of the project have freely

given their opinion that other sites for reservoirs could be easily procured by additional excavations to the tune of a goodly number of billion of gallons (five million cubic yards per billion gallons)!

It is certainly better, when within practical limits, to remove the loam from the surface of reservoir grounds near the water mark, although experience shows that inside of a very few years after flowage nature produces that result within the limits of fluctuation, except on flat grounds; but a general removal of the top soil is not to be advised.

Once in my experience, at a time when the public were under the influence of excitement produced by the temporary deterioration of the water supply, a large expenditure of several hundred thousand dollars was incurred in order to secure a perfectly clean bottom for a large reservoir. The principle was good, but although this occurrence took place a few years ago, I have failed to hear that the results are such as to justify the great outlay; the bottom, however, was so thoroughly denuded that although the foundations of the dam are very deep and well established, a large loss of water is produced by the general percolation through the ground.

As mountain streams cannot be secured for all communities, we must rest satisfied with what we can procure in many instances, *i. e.*, surface water, and turn our efforts toward the improvement of our water supplies. Before thinking of radical remedies, however, we ought to know better the causes of the evils from which we are suffering. We are in possession of the results of a large amount of very valuable research on the subject, and able men are constantly at work at it, but it seems that our efforts are not sufficiently concerted. Observations made at one spot under certain circumstances would acquire much more value if they could be compared with similar observations conducted elsewhere, and many individual efforts are wasted for want of the connecting links in the chain of evidence which could be furnished by others.

The results already obtained in this line of investigation by the Massachusetts State Board of Health in its efforts to make a rational and connected study of the question of water supplies throughout their State, indicate clearly what additional advantage could be obtained from extending the common field of research.

The subject is an important one to those whose fate it has been to wrestle with these questions in the face of the discontent, and even of the indignation of his fellow-citizens. It is a vital one and fit to be considered by this Society. I consequently suggest that a special committee be appointed for the purpose of corresponding with those who are most interested in the question of water supply for cities and towns, and of ascertaining whether it would be possible to join in a systematic study of these matters under concerted action.

Charles B. Brush, M. Am. Soc. C. E.—I have had some experience in this matter. First, in relation to the water of natural ponds. When

I was in the Adirondacks I took samples of water from the Great Lakes at a depth of about 60 feet and had them analyzed. I found that the quality of the water was not equal to the water we are supplying at Hoboken.

As to the remedy for algæ, I can only tell you what I have been doing. Our supply is rather complicated. We furnish water to nearly a dozen cities and towns of small dimensions as compared to New York, but they illustrate the difficulties which have to be met. I have found that the less I store the water, the quicker I deliver it to the consumer, the better is the supply. If I can only have a storage sufficient at any point for proper head and for relief in case of fire, then I have the most efficient storage for that particular town. We have a reservoir of some sixteen million gallons, and from that reservoir we supply, when the pumps are not working, all the little towns depending upon us. My practice during the past few years has been to put a tank on a suitable summit in each town and have an automatic arrangement so that the tank is always kept full. In such a case I have no trouble with algæ. One town had a reservoir built before the present company owned the works, and during eight or ten years there was at times complaint in this town as to the quality of the water from algæ. Since we have put in a tank and abandoned the reservoir there has been no further trouble.

I believe the reservoir was too large. For a time I supplied through the by-pass. While the water was rising in the reservoir there were no complaints, but they were renewed if there was a draft on the reservoir, especially if the water had been stored in the reservoir for any length of time.

The peculiar thing about algæ is that if a consumer tastes them once he imagines he is tasting them for several weeks afterward. On two or three occasions when complaint had been made I went to the town and found that the water was all right. I determined then to find out whether it was true that the water was bad in certain parts of the town and good in other parts of the same town. I gave directions as soon as the next complaint was received to have an examination of the town made and to locate the bad taste. I found after a little inquiry that the inspector simply took the word of the consumer. When the next complaint was made I immediately went up and took the inspector's report as he made it for the day and went to those houses myself and tried the water. In every case where they said the water was bad, I found that it was good. Then I called the consumer and asked him if he could detect any smell or taste in the water. He said he could not. When I asked why he had reported it as bad, he said, "Well, it was bad two weeks ago."

Now the first remedy, it seems to me, is to keep the water in motion. The next remedy that I found is to treat the reservoir purely and simply as an aquarium—to keep in it a proper equilibrium of animal and vegetable life. I have stored my reservoirs with carp, and after the

carp had gone, with black bass. I believe that in a reservoir, as in an aquarium, by having a proper proportion of animal and vegetable life, the water will be clean where otherwise it would be foul.

Mr. Fteley has spoken of the bitter prejudice with which this subject is treated. That is something in which I have had some experience. The Mayor of our city called public meetings, and I was informed that I was expected to explain why the water was bad, and to remedy it or suffer the consequences. Instead of considering the subject as a matter which was incident to water supplies generally, and which would have to be examined in a scientific way, it was treated with intemperance and ignorance. The prevailing theory at the first meeting was that the taste came from the cement that was used in the brick lining of the reservoir, and that the acids coming from the cement were the cause. Each meeting had its own peculiar advocate, equally intelligent and equally earnest.

It was absolutely necessary that we should do something; the algæ were there, and although I personally drank that water all through that period, and although I found that it did not injure me, still it was a most disagreeable condition of affairs. An expedient which was exceedingly simple was suggested to me by Dr. Leeds. We commenced to aerate the water, and we have been aerating it ever since. We did it first by simply pumping air into the mains. We have had no difficulty with water taken from the mains. Perhaps this was due to the fact that the water was in motion. Our consumers liked it, and we continued to aerate the water. We found that on certain days the algæ would be on the surface of the reservoir, not to the same extent as before, but to a certain extent. We then commenced to aerate the reservoir, and have been aerating it for the last two years. Since that time we have had no algæ. At times I have stopped when I have thought that there was a possibility of algæ coming in the water, and have let them accumulate. I then have turned on the air from the artificial springs, and the algæ disappear.

William E. Worthen, Past President Am. Soc. C. E.—Where do they go?

Mr. Brush.—I think they go to the air; I do not think they go to the bottom.

Mr. Worthen.—There is a difference of opinion about that.

Mr. Brush.—I do not care where they go so long as they go. I soon found that by keeping the water in motion I could keep it in better condition. As an experiment, during our trouble, I shut the water off from the reservoir. I wanted to see what it would do. Professor Leeds and I would go up and watch it. It was in bad condition for nearly two weeks, I think, and then it was bad enough. It had passed from the green scum to the bluish appearance, which is the most offensive condition. But after it had gone through its curing process it did not smell,

and there was nothing offensive about it. Some purging process is common to all bodies of water.

As to the question of diagnosis, I am a homœopathist myself, and have been all my life, and I care more for symptoms than for a diagnosis. When the symptoms appear I want to know what are the remedies. The remedies, I consider, are, keep the water in motion, keep the vegetable and animal life in proper equilibrium, and aerate the water. If there are any other remedies that do not injure the water I am anxious to know them.

Mr. Fteley.—In 1874 or 1875, when the City of Boston was looking around for a new water supply, a number of doctors—Dr. Edward S. Wood, Dr. Swan and Dr. Bowditch—were appointed to examine the waters of the various streams and lakes which had been indicated as best to serve as a supply for Boston. They went around and made an examination, and among the experiments they made was one that should be quoted in connection with Mr. Brush's remarks. They took some of the water, which was of a brownish tinge, water coming from a stream which flowed from a peat meadow, and in order to see what aeration and motion would do, they attached a bottle of it to the ceiling of their laboratory and let it fall, drop by drop, into a vessel on the floor. The change in the color of the water was very small.

Professor Leeds.—Mr. President, it surprises me to learn that a sample of water, after falling through the air, was not worse than when it started. I had occasion recently in some experiments on peaty water to filter water twice in the same manner. The amount of nitrogen was increased by dropping, drop by drop, through the air. It was filtered through a carefully cleansed filter of ignited quartz sand, and the increase of albuminoid ammonia in the filtrate was attributed to dust and impurities taken up by the water in falling through the air.

Mr. Fteley.—I was speaking of color and taste only, not of quality.

Emil Kuichling, M. Am. Soc. C. E.—Mr. Rafter's paper is one of much interest, not only to water works engineers, but also to hygienists and chemists, and every such carefully prepared contribution to the limited stock of exact knowledge on the subject which is now available in scientific literature should meet with a hearty welcome. Disagreeable tastes and odors are of common occurrence in public water supplies, especially in those cases where the source is impounded surface water; but while this fact is well-recognized, little attention seems to have been given to ascertaining the precise cause of such tastes and odors in the great majority of cases, and the public appears to be contented with the general assurance from physicians that no serious consequences to health have ever been observed to follow the free use of such water.

It must, however, be remembered that in absolutely pure water no form of life can exist for long periods of time, since the elements of

nutrition are then absent; hence the presence of life in a water indicates that the same is not entirely pure, and therefore it may be asserted that the amount and quality of living matter in a water-supply can be regarded as a measure of its wholesomeness. In the paper under consideration the discussion is nominally limited to algæ, although the author has introduced several species which botanists recently class with fungi and bacteria; but as the dividing lines between these three classes of thallophytic vegetation are not sharply defined, a greater scope is generally allowed to the first-named term so as to include both of the latter. To admit of a somewhat wider range in the debate, however, it is to be regretted that the author has not alluded to his researches in connection with the presence of *Zoophytes* and microscopic animal life in potable water, since *Spongilla*, even in small quantity, is known to have communicated an offensive taste and odor to large ponds and reservoirs.

As stated in the paper, the aquatic plants capable of producing disagreeable tastes and odors in both the water and the atmosphere above it are by no means limited to some of the colorless, bluish, or bluish-green microscopical varieties, but may belong to the grass-green families and even to other orders than algæ. Much depends upon the stage of growth of the plant whether it will develop such offensive qualities, and doubtless also the kind and amount of nutrition contained in the water play an important part in this respect. In the course of my examinations of certain shallow ponds in this State (New York) several species of coarse, flowering water weeds were found which emitted a nauseous odor when removed from the water, and which was highly intensified upon being crushed between the fingers. To make sure that this was not due to the presence of minute parasitic algæ, samples were submitted to a thorough washing and then to a microscopical examination, but, aside from a few diatoms, no other plants were found attached, and on crushing the specimen the same characteristic odor was perceived as before. It was therefore concluded that the odor emanated from the said coarse weeds directly.

Between the temperature and the development of these peculiar tastes and odors in a water supply, a certain relation is generally believed to exist, depending, however, upon the species of the vegetation. Sometimes the development occurs in winter and sometimes in summer, but so far as has been learned from the reports, the species of plants producing it in different seasons are not the same. At Rochester, N. Y., the water supply from Hemlock lake was thus affected during the warm season in 1876 and during the cold season in 1888; in the former case, the cause was ascribed by Professor Lattimore, of the University of Rochester, to some obscure condition of a variety of minute algæ, without specifying any particular species, while in the latter, the author and his associates lay the blame upon *Volvox*, as set forth in the excellent paper. It should be remarked that in the list of algæ given in Pro-

fessor Lattimore's report on the condition of the water in 1876, *Volvox* was not mentioned. In other localities where surface waters are used as a source of supply, the difficulty is said to be regularly experienced as soon as the water in the reservoirs acquires a temperature of about 70 degrees Fahr., thus indicating that the same cause is in operation.

With a rise in temperature to above 45 degrees Fahr., the common bacteria found in all surface waters begin to increase rapidly, and generally attain their maximum rate of development when the water reaches its warmest state in open reservoirs, provided that the necessary quality of nutrition is available. A marked reduction of temperature, however, kills off many varieties after they have reached a certain growth, and in view of the enormous numbers of such organisms often found in this kind of water, it may be possible that the offensive taste and odor sometimes result from their decay. It is needless to say that the ordinary water bacteria here referred to are not recognized as being in any sense dangerous to health, and must not be confounded with the disease-breeding varieties; but as several eminent bacteriologists have recently advanced the proposition that, in general, the quality of a potable water stands in inverse ratio to the number of bacteria contained therein, the subject is mentioned in the hope that it will lead to further investigations in this direction.

The theory of Mr. Rafter that the final cause of the odors and tastes in a water supply must be sought in certain secretions of the algæ at different stages of their growth, is very ingenious and plausible, and opens the door to a new line of inquiry, analogous to that relating to ptomaines. The putrefaction of gelatinous and starchy liquids is attended with a peculiarly nauseous odor, and sulphur will form a variety of unstable and volatile compounds with some of the other elements contained in the water or the plant; as to the oily matter thus secreted, little concerning its character appears to be as yet known. It is to be hoped that on the completion of his investigations Mr. Rafter will kindly communicate the results to the Society in another paper.

In conclusion, it may be remarked that, inasmuch as the author has fully set forth the essential facts relating to the most interesting cases of the contamination of a public water supply by algæ, which I have had an opportunity of studying personally, and as reference to other kindred pollution is not in order, little further remains for me to say in the premises.

Dr. J. Edw. Line.—The statement credited to Mr. Fteley by the late Professor Nichols, that common salt added to water having a given vegetable taste develops therein at times a decidedly oily flavor, suggests a few observations pertinent to the discussion.

The dissociative effect of common salt is seen in the readiness with which the oily matter of the skin, also its superficial epithelial cells, are removed in the bath; in the separation from the teeth and neighboring

soft parts of salivary and mucous deposits and particles of food, thereby rendering the oral secretions miscible in water; in the separation of the globules of milk and the disks of blood; in the loosening up from each other of the fibers of such tough animal tissues as the cornea, sclerotica, tendons, fasciæ; in the disintegration of masses of vegetable matter floating in water; in effecting the discharge of the contents of decayed, broken or otherwise ruptured vegetable cells, as in the algæ.

This dissociative effect, studied microscopically, seems to be due to the induced shrinkage of the cells and fibrous components of the mass, whether animal or vegetable.

This shrinkage is toward a common center, the contour making a closer approach to modifications of the sphere than normal. Angular bodies lose their angularity, and bodies that are oblong shorten and thicken, as may be especially remarked in the oblong-celled algæ. In assuming the forms in question, the cells, fibers and filaments are torn asunder, and part permanently with their inherent tendency to aggregation.

With this dissociation we have large increase of surface upon which oxidizing and other reducing agents act to such a degree that there is marked intensification of taste or odor, or both, where before they were hardly or not at all perceptible. Briefly, the intensity of the taste or odor, or both, of masses of animal or vegetable matter is directly as the surface exposed to the reducing agent.

In the case in question, assuming the oily flavor to have had its origin in the oily matter found in the cells of certain algæ (*Vaucheria*, for example), the application of a solution of sodium chloride would disintegrate the masses, dissociate its elements, thus liberating the oily matter that had escaped from cells mechanically ruptured, or whose investment had been destroyed by decay, causing the cells to empty themselves of their contents into the surrounding medium.

This is easily demonstrated microscopically. In the case of *Vaucheria*, the oil globules may be observed in the cells and between the filaments, and on the addition of sodium chloride they are seen to escape from beneath the filaments and through the holes and fissures of the cells, suggesting an interchange between the medium in which the mass is floating and the denser contents of the cells, its oily and granular matter participating in and indicating the direction of the current.

Discussion at Convention.

Geo. W. Rafter, M. Am. Soc. C. E.—The paper on fresh water algæ has already been read at the regular meeting of the Society of May 1st, and I will therefore speak very briefly at this time. I desire to say that this matter has been with me, in the fullest sense of the word, a leisure-hour work. I have been in the predicament of having to do a day's work every day, and then if I cared to amuse myself in the evening of working at this question of the algæ. The result is, there are a few things I have found out and many that I hope to find out. The paper must be taken, not as indicating complete data as to the algæ in relation to the purity of water supplies, but, rather as a series of surmises, of which I have introduced only those appearing the most probable. My facts, of course, so far as I have introduced facts into the paper, I think are reliable.

I found at the beginning of this little side study that it was exceedingly difficult to get any information about fresh water algæ from the sanitary point of view. Scientific people have studied the question almost exclusively from the classificatory point of view, so that including the forms which are properly classified as fresh water algæ both in this country and abroad, there are catalogued, illustrated and described at least ten thousand forms.* Of the greater number of these, absolutely nothing is known from the point of view which we are now considering; the life history of many is still entirely unsettled. The field of work is, therefore, undoubtedly very large, and there are probably many questions of development which will bear on the final solution.

I also found, on consulting the limited literature of the subject, the opinion quite general that only a few species were likely to cause trouble, whereas the real fact as stated in the body of the paper is that the number is large. That these troubles usually occur in the summer or fall is due chiefly to the fact that the latter part of summer and the fall is the fruiting period with a considerable number of species, the period of complete development of the process of reproduction, and it is inferred that in many cases the objectionable tastes and odors are associated with the reproductive period, and while I am unable to prove the proposition, I am nevertheless quite certain that in some cases these tastes and odors are really the result of the reproductive process purely, and are entirely independent of the question of decay. In other cases it is equally certain that decay is the sole cause of the trouble, while in a third class, inasmuch as decay frequently follows closely on fruiting, it will be found that both are concerned in the trouble.

The question of ground water supplies has become a prominent one in this country and is likely to become still more so in the future.

It is common experience that there are parties going about the

* This includes the desmids and diatoms, the diatoms being by far the greater number.

country who, while without scientific attainments, are, as a matter of business, advocating the superiority of ground water supplies for any and all locations, the object of this advocacy being of course the sale of patented devices for obtaining such a supply. I wish to call the attention of these gentlemen, particularly, to the untruthfulness of their stock statement, that no growths of any sort can occur in ground water supplies. Undoubtedly, supplies of a high degree of purity can be obtained in many instances from the ground, but its advocates should remember that such a supply is not only essentially experimental but that it almost invariably imposes troubles peculiar to itself, which must be met and remedied frequently by considerable expenditure. This has happened both at Berlin and Jamestown, the two cases cited, while Professor Leeds has called attention in a paper before the New York Academy of Science * to the expense incurred at Greenwood Cemetery, because of offensive growths in water from driven wells. We are therefore sufficiently advanced in accurate knowledge of this particular phase of our subject to affirm with a considerable degree of positiveness (*a*) that a covered reservoir is always necessary in connection with ground water supplies;† (*b*) that such a reservoir will prevent algal growths provided *Crenothrix* is not present, and (*c*) if *Crenothrix* is present there is as yet no known remedy except to send the water to the consumer immediately on taking it from the ground or else to resort to expensive sand filtration or possibly artificial aeration, and inasmuch as it is frequently impossible to send the supply to the consumer at once, it follows that a ground water supply *may* impose the expense not only of covered reservoirs but of either artificial aeration or filter heads as well. Nevertheless, I should not hesitate, as an engineer, to recommend a ground water supply in any place where a decent consideration of all sources clearly indicated a minimum total expenditure for the ground water supply, questions of quality also receiving due consideration. My only object in introducing this phase of the question at all is to indicate to the advocates of the various driven well and other similar projects that they have hardly given the matter sufficiently broad consideration.

In this connection, I may say that I am not yet satisfied that artificial aeration will prove efficacious in every place, although the arguments which Professor Leeds has given elsewhere ‡ are strong and deserving

* The American System of Water Purification, by Professor Albert R. Leeds, Ph.D. Reprint, New York, 1887, pp. 11-12.

† I of course do not overlook the fact that ground water supplies may be constructed without any reservoir at all, *i. e.*, direct pressure, but such construction usually implies expenditure in excess of cost of reservoirs, either in additional first cost of plant or in capitalized cost of expense of operation, so that it may be affirmed that the cases are exceptional in which a rational design for a ground water supply would not include either a reservoir, standpipe or elevated tank of some description; at any rate the assumption made is sufficient for the purposes of the present argument.

‡ The American System of Water Purification, and Reports on Present and Future Water Supply of Philadelphia, in the Annual Reports of the Philadelphia Water Department for 1884 and 1885; pages 353-381 of Report for 1884, and pages 379-400 in Report for 1885.

of very careful attention. We must not overlook, however, the fact that many of the algæ flourish best under what can be appropriately termed conditions of ideal natural aeration, as for instance, on rocks constantly wet from dashing spray, in running brooks and other similar locations, and these, too, are mostly forms which can be classed as aquatic rather than aerial. It is quite probable, therefore, that the case may arise in which artificial aeration will lead to increase of the difficulty which it is intended to remedy.

The opinion has been expressed in the course of the discussion that circulation will prevent the formation of the objectionable algæ. Upon this point as well as the preceding one I cite the following: In the latter part of April, 1889, I had occasion to examine a rapid running mountain stream in Central Pennsylvania, with reference to its value as the source of a water supply for one of the towns of that region. The stream is fed by springs flowing from the base of sandstone ridges, and topographically and geologically is an ideal water supply. An analysis made at the time by a Philadelphia chemist resulted as follows (results in parts per 100 000):

Solid matter upon evaporation to dryness	4.84
Silica (SiO_2)	1.72
Alumina (Al_2O_3) / Oxide of iron (Fe_2O_3)	0.19
Lime (CaO)	0.54
Magnesia (MgO)	0.25
Sulphuric acid (SO_3) in combination	0.29
Chlorine (Cl) in combination	0.50
Free ammonia	Trace
Albuminoid ammonia	0.005
Nitrogen in nitrates	0.068
Nitrogen in nitrites	0.0003

From the point at which it was proposed to erect a dam and form an impounding reservoir to head of stream was about one mile, in which distance the stream fell at least 150 feet. The feeding springs were few in number, and each contributed a considerable volume to the flow. On April 22d, every point along the stream where algæ could find any chance to grow was swarming with them, and in one dozen collections made at various points, all within one mile, the following were identified:

Spirogyra,
Zygnema,
Ulothrix (3 species),
Tetraspora,
Vaucheria,
Oedogonium,
Fragillaria (2 species),
Synedra.

All of these were in an active growing state, and a mill pond further down the stream was nearly overrun with them. In this stream, in which flowed ground water of great purity, at any rate a process of natural aeration was going on far superior to any artificial process that could be

devised, and nevertheless the algæ flourished abundantly at the same time. The water however was deliciously pleasant and I had no hesitation in recommending it as being a desirable source of supply.

The clew to the reason for a vigorous development of algæ in this stream is undoubtedly furnished by the chemical analysis, from which it is learned that these waters contained quite an appreciable amount of inorganic nitrogen, and this would act as a stimulant of algal life. In such cases we are compelled to admit, however, that the algæ are in no sense purifying agents, but are, on the contrary, the cause of some deterioration.

In the same vicinity a spring was visited from which the flow amounted to several hundred thousand gallons in twenty-four hours. Nevertheless, the margins were thickly coated with a vigorous growth of diatoms. The analysis of this water stood (results in parts per 100 000):

Solid matter upon evaporation to dryness	15.76
Silica (SiO_2)	1.30
Alumina (Al_2O_3) / Oxide of iron (Fe_2O_3)	1.10
Lime (CaO)	7.83
Magnesia (MgO)	0.54
Sulphuric acid (SO_3) in combination	0.07
Chlorine (Cl) in combination	0.93
Free ammonia	0.003
Albuminoid ammonia	0.003
Nitrogen from nitrates and nitrites	0.38

Relative to artificial aeration, therefore, it appears that we are warranted in concluding that benefit will probably follow its application, in case water needs to be washed free of an odor; we may also expect benefit in sewage contaminated waters where bacteria are materially assisting nitrification. The experience of Mr. Brush at Hoboken and Greenwood Cemetery further indicates that it will sometimes assist in getting rid of the algæ, but that it will certainly be efficacious in every possible case, is still an unsettled question.

J. James R. Croes, M. Am. Soc. C. E. (the Chairman).—In an introduction to the discussion of matters relating to the purity of public water supplies, a few remarks on the general subject of the nature of the investigations referred to in the paper under consideration may not be inappropriate for the information of those present who are not familiar with the subject. We are all aware that at certain times most water supplies drawn from reservoirs, whether natural or artificial, are subject to a species of disease, rendering the taste and odor of the water offensive for a time. The conditions under which this disease has made its appearance have been so various, so far at least as examination by unassisted vision and observation could disclose, that the causes of the disease have

remained obscure. Carefully framed theories, based upon the conditions found to exist in one case, have been completely overthrown by observation of other cases, where the same apparent conditions existed, but the results were entirely different.

Chemical analysis alone also failed utterly to discover the causes of the disease.

In the general progress of microscopical and chemical investigation during the last few years it has become evident that the influence of the infinitesimal and to the unassisted eye, invisible, forms of animal and vegetable life with which nature abounds, on those organisms which from their greater size we look upon as the superior ones, is far greater than had been supposed. The generation, growth and decay of microscopic animals and plants have been found to produce, or at any rate to be coincident with, certain phenomena which previously had been supposed to be contingent upon or caused by chemical conditions for the variations in which no adequate cause could be assigned. The microscopic investigators of animal forms are striving to determine what species of bacteria are certain to produce injurious effects on human beings when taken into the system.

So the investigators of vegetable forms are seeking for evidence as to plants, which can be proven to cause certain unwholesome or unpleasant effects on the human race, and then how the growth of such species can be prevented, or at least the ill effects of their perfection and decay can be obviated. It is to this branch of the subject that the author of the paper now before us has devoted his attention.

By comparison of observations in many different places at different times, it is considered to be almost certainly proved that the unpleasant taste and odor referred to is to be attributed to algæ in the waters. These algæ are defined as flowerless plants, having no proper distinction of stem or leaf and always growing in water. They are generally thread-like structures of a more or less slimy consistency. Many of them are mere masses of jelly. They are divided into three great families, which are distinguished by their color, very much as races of human beings are, and as in the human race, there are also marked structural peculiarities attending families and habitat.

The nomenclature of the various classes, genera, species and individuals, is in that curious combination of mutilated Greek and Latin which is adopted by scientists, especially by microscopists. The long names which, at first, are somewhat appalling to the student of the subject are found on a little investigation to be derived generally from the Greek, and to denote some physical peculiarity of the class or individual, or to commemorate the person or the place by whom or where it was first discovered. Thus, the classes, *Cyanophyceæ*, *Rhodophyceæ*, and *Chlorophyceæ*, mean simply, respectively, blue, red and green seaweeds. The diatoms are composed of single cells w th walls of silex (*Gr.* διατεμνω,

to cut in two) while the desmids have no silicious walls, but are grouped together (*Gr.* δεσμη, a group) and the joints connected.

Of the subdivisions and individuals, there may be mentioned the *Schizosporeæ*, indicating plants whose spores are splinter like, the *Hydrodictyon*, which is Greek for a water-net, and the *Chrenothrix*, which signifies a water-hair, from its filamentous appearance, and its suffix, *Kühniana*, tells us that Dr. Kühn first described and individualized it.

Among the most curious of the origins of names is that of the *Confervoids*, which dates back to the time of Pliny, who called the water-sponge of that day *Conferva*, because it was said to possess the power to heal (Lat. *conferveo*) broken bones. The *Clathrocystis*, which Mr. Rafter mentions, means simply a closed bladder, while the *Cœlosphœrium* is a hollow sphere. But while this branch of the study may be interesting to some, it does not add to our knowledge of the habits of the plants, and I will ask one of the most acute and careful of the observers of these phenomena to give us some of the results of his studies, and will call on Mr. William E. Worthen.

William E. Worthen, Past President Am. Soc. C. E.—Most engineers connected with the water supplies of towns and cities have had their attention called to the presence of fresh water algæ in stored water, have had some experience in removing them, and have studied chemical and biological literature on the subject, and have generally come to the conclusion that in themselves algæ are neither dangerous to life nor detrimental to health, but that in life they are objectionable in color, and dying they give offense in odor and taste to the water.

In 1869 I put in the water works for Long Island City; the supply was from a well 50 feet in diameter, paved at the bottom with wall stone, with open pipes driven some 20 or 30 feet lower. On one occasion, before the well was covered, on a very bright still summer day, the surface of the water was covered almost instantly with a coat of brownish algæ, which continued nearly all day, but a fresh breeze sprung up, the sky became cloudy, and they suddenly disappeared. It seemed to me that they detached themselves from the rock paving, rose, and as suddenly sunk and again covered the rocks. The well was soon after domed over, and I have no record of the reappearance of the algæ. The algæ bruised in the hands gave no positive smell, analyzed chemically, no different constituents from former analyses of the water.

I had a similar experience with the spring in the ditch at Fort Wadsworth, S. I., from which the water supply of the fort is taken, the covering of the well stopped the floating algæ.

The supply of East New York is from wells, with an open reservoir of some three million gallons, for use in cases of fire or accident; here the algæ gave a distinct green tinge to the water, but without any offense in taste or odor. From some source the water flea came in, and it was

objectionable, as it was visible, but they were interesting as a study, and I kept them in a bottle for some weeks. Lime did not affect them, except as it precipitated their pabulum. The algæ grew and then faded out gradually. The same flea I have found in an underground reservoir which received its water from roofs.

In many large wells or filtering reservoirs, exposed to air and light, I have seen beautiful developments of algæ, even with the temperature of water at 52 degrees. My idea is that they supply oxygen to the water in which well water is deficient. But in all still waters exposed to air and light there are always more or less algæ, but without offense in taste or odor, if in a healthy and live condition.

Professor William Ripley Nichols says (Report on Cochituate Water, October 1st, 1887): "The peculiar tastes and odors are often noticed where the animalcules are comparatively scarce;" Mr. Burgess, in the sub-report: "The entomostraca are scarce in our water supply this season;" in allusion to which I suggested to the Professor that it was the funeral of these mites that the water-takers were attending. That they are objectionable to sight when living, and to taste and smell when dead, there is no doubt, but what are we going to do about it?

Can Water be Disinfected?

A Committee of the American Health Association (Vol. XIII, 1887) states absolute disinfectants to be but few. 1st, fire, burning, boiling, baking; 2d, chlorine. None applicable to a domestic water supply except boiling in small quantities and drank without contact with air, water or ice; but even boiling does not destroy the active principles of poisonous mussels nor the poison of typhoid fever.

Freezing will not always destroy fish life; my friend, Mr. A. J. Rossi, froze gold fish stiff in a cake of ice, which, melted out slowly, regained their former activity. And ferments continued to give off bubbles whilst ice cakes were being formed.

M. Melsons ("Annales des Genie Civil," May, 1870) filled a bomb shell with water and yeast and burst the shell by freezing, without destroying the yeast, showing that its vitality would resist both cold and immense pressure.

The development of germs may be checked by freezing, but under proper conditions they regain their activity. Dr. Cohn, of Breslau, states that a single bacterium would be able to fill up the whole ocean with its progeny in less than three days if only sufficient food and proper temperature were given. It is evident from all experience that it is impossible to annihilate all the germs and spores.

In the Minutes of the Proceedings of the Institution of Civil Engineers, July, 1886, will be found a communication on Water Purification, with an extended discussion on the same, which is so far conclusive against the possibility of removing entirely in any way these micro-organisms,

or even checking their growth, except temporarily. Filtration, except with great care, may increase rather than diminish the number of such organisms, and filtered water unless used at once becomes more offensive than the unfiltered; filtered water for ship's use was a failure. At Poughkeepsie, at certain times in summer, water must be used directly from the river.

The purification of water by agitation with solid particles, as coke, charcoal, chalk, or by precipitation, is only temporary in its character; the organisms carried down soon become redistributed.

In view of the universality of these micro-organisms and the impossibility of their destruction or removal, is there any necessity of either? Not certainly as to their danger to life or health.

Professor William Ripley Nichols, in "Water Supply:" "Their presence is not a sign of contamination, as they occur in natural ponds removed from all polluting influences."

Dr. Klein: "Septic organisms are absolutely inimical to pathogenic organisms, and not only are they inimical, but so much stronger, and their vitality so much greater, that it is not possible for disease germs to exist in their presence."

M. Bechamp ("Annales des Genie Civil," January, 1870): "M. Pasteur denies that sound wines contain living ferments, that those ferments only which spoil wine are living. I, on the contrary, say that both are living and capable of acting physiologically to produce chemical actions known under the name of fermentation. I am convinced that the cause which gives age to wine and occasions rapid transformation is not the destruction by a degree of heat beyond the capacity of endurance, but is an exaggeration of their function or direction in a determinate way. I believe that the whole secret of the art of giving age to wines and preventing them from spoiling consists in favoring the production of beneficent organisms."

Dr. H. P. Walcott (Public Health Association Address, 1886): "There is need of careful observation and experiment on the side of the life history of these organisms in natural conditions, not under the artificial arrangement of sterilized chambers, but in the presence of the uncounted superfluities of life ever present in the atmosphere we breathe, in the water we drink and in the ground beneath our feet. The phenomena of microscopic life are not likely to be found essentially different from those observed by the agriculturist."

With all due respect for chemical analyses, for I have them made of waters proposed for town supply, I investigate biologically the living visible fauna and flora of such waters and their surroundings and conditions of health. For many years I have been collecting magazine scraps on aquaria and in this line would refer to the Reports of Commissioner Thomas B. Ferguson, Paris Universal Exposition of 1878, as most valuable, and affording hints by which the condition of the water

may be judged and how the growth of forms and life may be modified and controlled.

The great principle to secure healthy water is to follow the teachings of the aquaria, keep up the circulation of the water and aerate it, favor the production of beneficent organisms and preserve the balance of animal and vegetable life. Death in its natural course by absorption of micro-organisms into those of a higher growth is inoffensive, and without a microscope we can judge of these last, we know a healthy plant or fish and feel assured that the water is good that promotes healthy organisms; "all vegetable aquatus, mostly microscopic, are requisite for the continuous oxygenation of water, and possessing a solubility much greater than that of the oxygen drawn from the atmosphere;" but there may be life in excess of what can be absorbed in the higher grade; study to keep down excesses in all forms looking largely to air and light to preserve the balance.

For reservoirs for well waters I recommend those of small capacity, only such as may be sufficient for a night supply or for contingencies of fires or the breakage of machinery. For reservoirs of streams make the distributing ones small and let the water flow down to them from the storage reservoirs, through open and rippling channels, keep up the circulation and give good chance of aeration.

Make the storage reservoirs ample and numerous, avoid the necessity as far as possible of changing the level by drawing it down largely. As far as they are drawn down keep the bottom clear of vegetation, as vegetation exposed rots and becomes offensive, contaminates the water.

When water has become offensive time is an element for the removal of the objectionable taste and odor by the oxidation of the effete matter, which will take place gradually in open reservoirs, but more rapidly by exposure to light and air in flowing streams with dams and ripples. Dilution may modify the offense, but a very slight taint makes water objectionable for domestic purposes, and boiling seems to intensify the offense. If there are a number of reservoirs, shut off the offensive ones for a time to give as long and broken flows to the water as possible from the reservoirs to the mains or closed aqueducts. Water sheds, reservoirs, streams, channels, aqueducts, mains, &c., in fact everything connected with a water supply should be under an intelligent supervision and management.

James B. Francis, Past President Am. Soc. C. E.—This matter of circulation is, I think, fundamental. My own house is supplied with water from a pipe connected with a reservoir containing, usually, rather less than two millions of gallons, the pipe serving both for supply and distribution, that is, the water in it is sometimes flowing to the reservoir and sometimes from it. The reservoir being small in proportion to the supply, the water in it is in constant circulation. My neighbors are supplied from a reservoir containing about thirty millions of gallons, and

complain, at times, of a bad taste in the water. I never have such trouble; both supplies are from the Merrimac River, and the only cause of the difference in the quality of the water, that I can discover, is in the difference in the activity of the circulation in the reservoirs.

FREDERIC GRAFF, Past President Am. Soc. C. E.—I am a firm believer in what my friend Mr. Worthen has said as to the value of circulation. I think that we may all see that water supplies taken from running rivers which have little or no pollution thrown into them from sewage are certainly much freer from any troubles of the kind indicated than water supplies taken from large impounding reservoirs where the water may stand in an almost stagnant condition for a long time.

In the utility of aeration, I agree with Mr. Brush. Of course, it is a very serious matter to do this on so large a scale as most of our cities now require, but when it can be done, I think it may be a cure for very many troubles.

I adopted it to a limited extent about the year 1872 in the Belmont Reservoir of the Philadelphia Water Works, by carrying the pumping main up vertically above the surface of the reservoir through the center of the division embankment, and then allowing the water to discharge over the lip of the main, which was enlarged to 39 inches diameter, forming a jet, so to speak, of that diameter, thence flowing into a basin 9 feet in diameter flowing over the circumference of that, and then passing into a reservoir with two overflow sides of about 20 feet long each, over these down the sloping brick-lined sides of the reservoir proper, until it reached the surface of the water therein, thus exposing all the water pumped into the reservoir through this main in thin sheets to the action of the air in its passage into the reservoir.

All the water supplied from that reservoir could not be thus treated, as only one of the pumping mains could be so arranged, but six millions of gallons passed into the reservoir daily in this manner. We never had trouble with bad water when it was in use. As far as I know, this was the first application of the overflow method of aeration used in this country.

I am a firm believer in aeration of water, and in methods of producing it by circulation, artificial or natural. I think that Rochester adopted a similar method of passing the supply from a storage reservoir into the distributing reservoir through jets, with somewhat the same object as I have spoken of.

MR. RAFTER.—Yes; the fact of the matter being that the water discharged from the conduit into the distributing reservoir is simply fed into it by jets. When we get to the limit of our supply we are not able to use the aeration scheme.

MR. GRAFF.—I understand it is effective when in use.

MR. RAFTER.—Fairly so; yes.

Mr. Croes.—In drawing the water from the storage reservoirs in the Croton valley arrangements are made by which the water is aerated on leaving the reservoir; that is, it is discharged into the stream through a jet, thrown up into the air before it falls down into the stream.

Mr. Fteley.—There is such a thing in existence. I have seen the apparatus and I know it is used, but whether continuously or not I do not know. Mr. Cooper can tell us about that.

S. L. Cooper, M. Am. Soc. C. E.—The storage reservoirs in the Croton basin are provided with jets through which the water is drawn in the form of fountains.

The first year that the water was drawn from the Drewville reservoir, on the Middle branch of the Croton river, the gases set free at the fountains were so foul that remaining long in the neighborhood was very undesirable; and a watchman's house near by painted a light color was almost turned black by the gases.

After the water passed through the fountains, and had run down the stony bed of the Croton river for a few miles, the bad odor and taste had quite disappeared. This is a remarkable instance of the effect of thoroughly aerating water.

The trouble in the case was no doubt caused by the decomposition of the vegetable matter in the bed of the reservoir, which had not been removed before the reservoir was filled with water.

Mr. Croes.—At the outlets of the large artificial impounding reservoirs on the upper waters of the Croton, at Boyd's Corners on the West branch, and near Brewster's on the Middle branch, Mr. E. H. Tracy, then the Chief Engineer, had the outlet pipe carried some distance below the dam to a basin, the pipe terminating in a number of vertical nozzles, so that the water was thrown up in a number of jets when the reservoir was being drawn from. I understand that Mr. Cooper's statement of the effect produced, namely, that the bad water was aerated in that way and lost its taste in passing down the stream is quite correct.

Charles B. Brush, M. Am. Soc. C. E.—At the risk of some repetition of what I have said before in the discussion of this subject, I would like to call your attention to certain points.

In the first place, I think we have arrived at that stage where we can say that all water supplies are much better at some seasons of the year than at others, and that the diagrams we have here indicate the better and worse conditions of the water at different seasons and under different climatic conditions. Every river has its purging periods. On the supply of which I have charge, we have analyses of the water taken every month, both at the intake and in our distribution pipes and reservoir, and we find that we can plot a regular curve every year, showing that at certain seasons of the year the water is in better condition than at other seasons. The better seasons with us are in the fall and spring;

the worse mid-summer and mid-winter. The drainage area of the supply to which I refer is entirely free from all source of pollution in the shape of sewage. It contains no large towns and only a few scattered villages. The latter do not drain into our source of supply except by filtration and at a considerable distance from the river. The state of the water is due purely to natural causes. Those causes have produced a condition of affairs which was a very serious matter. In the year 1884 the algæ commenced to appear in our reservoir, in the shape of a fern-like appearance, which extended down below the surface about 5 or 6 inches. After a while this fern-like appearance commenced to grow thicker, until it took the form of a green paint on the surface of the water. At that time the smell and taste were very marked; they were so marked that the houses having this supply found it undesirable to use for domestic purposes. The green paint condition continued for a week or ten days, passing into a bluish color for another week, and soon after the water was perfectly good. It cured itself. During the trouble I fcund that the water entering the reservoir in motion was in a very much better condition than the water that remained in the reservoir, and therefore I shut the reservoir off and turned the water directly from the pumps into the city. I was exceedingly anxious to know whether that water was going to create sickness, and consequently, during the whole time these algæ were in operation I drank the water; it did not make me sick; I do not believe it will make any one sick. It is nauseating, and that is the worst feature of it. I think that the condition of water, in a case like that, shows its worst on the face of it; I do not think it will create anything like epidemic sickness.

Something had to be done. As soon as the condition of affairs that I have described began to take place, I called in Professor Leeds of the Stevens Institute. He is the chemist of the company, and he made analyses of the water during the whole time of the trouble. One thing that we noticed in connection with it, was that the supply of oxygen in solution was very low; it was one-third of 1 per cent. instead of two-thirds of 1 per cent , its normal condition. The thought occurred to Professor Leeds that aeration would be a good thing. We commenced aerating on a large scale. In the West they have been in the habit of having some kind of contrivance with a bucket on it, and when the water is in bad condition they churn it around, and in a little while the water will be perfectly clear. The best water is that which comes from mountain streams, and the nearer we can get our supply in that condition—broken up—the better it is. We forced fresh air in our pipes in 1884, and up to this time we have had no repetition of that trouble. I do not mention this as a universal cure-all, but I do say it has helped us out. We are now aerating the water in our reservoirs as well as in the pipes. The people see that air bubbling up in the reservoir. It looks like natural springs, and creates a favorable impression.

In some cases I have let a little of this green paint formation grow by stopping my aeration, and it has collected in a corner of the reservoir. When I turn on the air the algæ disappear.

Water becomes impure from natural conditions, from meteorological causes and local phases of animal and vegetable life. I believe with Mr. Worthen, that a reservoir should be treated as a large aquarium. I stock my reservoirs with carp and black bass, and with aeration I am able to keep the water in good condition; but whether that will always continue, or whether there will not conditions arise which the means I am now using will not be sufficient to counteract, I cannot say. It is perfectly evident that it is of the most vital importance to get at the bottom of this trouble. I do not believe that the algæ and the bacteria are the real difficulty. I believe that they are simply the results; that they are the indicators of the things which are wrong. I think that the attempt to physically remove algæ or bacteria is a mistake. I have tried that myself. When the green paint appeared on our reservoirs I was determined that our consumers should not see it; I had it carefully removed, but the next morning the green paint was all there. What I have tried to do is to cure the conditions of which the algæ and bacteria are the indicators.

Mr. Worthen.—Would you get rid of them entirely if you could?

Mr. Brush.—No; there are certain forms which indicate conditions which should not exist; but as for getting rid of animal and vegetable life altogether, no. I do not think we want for domestic supply water that is entirely pure. Distilled water is not desirable. I think any water that has been filtered is liable to deterioration very quickly; the mere fact of filtering the water takes the life away from it; if you are going to use it immediately, very well.

When a water supply is affected there is a popular excitement which is hardly understood except by those who pass through it. If consumers cannot get their meals and their baths when they want them, the people who supply the water have to encounter a storm which is not a very pleasant one. In this particular case of mine I was cited to appear at public meetings and given to understand that I would have an opportunity to explain matters, and if I could not do so satisfactorily the result would be on my own head. The excitement was very great. One man, who claimed to be a chemist, gave a long description of the troubles and the conclusion he came to was "that, in building the reservoir, cement had been used and that the cement in the water created the difficulty; if we had not used cement it would not have occurred; that I ought to have known better." Of course there are funny sides to a thing of this sort, but there are very serious sides also. Those who are weak and sick, and ladies and others who are easily frightened, immediately feel that the only thing to be done is to leave the city as soon as possible.

I think it is exceedingly important that there should be somebody to whom people could refer for information on the subject, and thus check the reckless statements which appear from time to time.

You have already heard from Mr. Rafter how carefully he has been looking into the matter. I, myself, was perfectly astonished to learn what they were doing in Massachusetts; their reports, I believe, have not yet been made, but next year they will be issued, and thus we will have the results of their labors.

Samuel Whinery, M. Am. Soc. C. E.—Mr. Brush alluded to a device that is in use in the West for aerating domestic water supplies and perhaps a few facts in relation to it may be of interest.

In many sections in the Mississippi Valley the supply of water for domestic purposes is collected from the roofs of buildings and stored in cisterns. These cisterns are practically small storage reservoirs, constructed by excavating a space under ground of the required form and capacity, lining the walls with brick and cement mortar, and covering the top with an arch or dome of the same material.

It is found to be a fact, notwithstanding that the water so stored is originally pure, and that the cisterns are frequently cleaned out, that this growth of algæ frequently occurs in them, rendering the water offensive to the taste and smell.

It is worthy of note, in this connection, that in these cases the development of this plant growth occurs, notwithstanding that these cisterns are covered over. It is found that if a pump is used in these cisterns that thoroughly aerates the water, this offensive odor and taste does not occur, and if such a pump is put in a cistern already offensive this trouble is soon remedied.

The most popular pump of this kind is a modification of the old fashioned chain pump, having for buckets small metallic vessels, holding about half a pint. The bottom of each bucket is perforated with a very small hole, and as the descending bucket enters the water in its inverted position it carries down with it into the water a volume of air, which gradually escapes in small bubbles, which rise to the surface, thus thoroughly aerating the water.

It is a very simple device, but has proved very effective, and cisterns provided with such pumps never become offensive from these growths.

Frederic P. Stearns, M. Am. Soc. C. E.—Mr. Rafter has referred to the experience with *Crenothrix* in the water supply of Berlin, and I think it may be of value to add the experience with this organism in the ground water supplies of Massachusetts.

There were in this State in 1887 fifty water supplies taken from the ground, which may be classified as follows:

Springs	16
Large wells	16
Tubular wells	7
Filter galleries	7
Filter basins	4
Total	50

The line of separation between the different classes is somewhat indefinite; but it is sufficiently exact for the purposes of this discussion.

When water comes from a natural spring or where it is filtered by some very slow process of natural filtration, the ground-water before exposure to the light is nearly free from nitrogenous organic matters, as is shown by the small amount of free and albuminoid ammonia found by chemical analysis. It is quite common to find about the following amounts of these constituents in parts per 100 000:

Free ammonia	.0002
Albuminoid ammonia	.0015

In ground-waters of this class *Crenothrix* has been found frequently in very small numbers, but it has never increased to such an extent as to give trouble, although it has been carefully watched for, particularly in districts where the ground contained much iron.

In several places in Massachusetts a filter gallery has been built on the shore of a pond or storage reservoir just at the high water line, or in some instances so that the gallery is buried to a depth of a few feet below the bottom of the pond or reservoir. In these cases the water is imperfectly filtered and contains after filtration a large amount of nitrogenous organic matter, the free ammonia being increased by the filtration and the albuminoid being decreased, as will be seen by the following typical case:

	Water from storage reservoir.	Water from filter gallery.
Free ammonia	.0004	.0206
Albuminoid ammonia	.0396	.0248

There are four instances of this kind in the State which have been carefully examined, and at each place *Crenothrix* grows in great abundance. In the worst case, during the hot weather in summer, the water contains so much of the *Crenothrix* coated with iron that it is very unsatisfactory for laundry purposes on account of the iron stains produced. The rapidity with which this organism multiplies as the water is passing through about two miles of pipe from the filter gallery to the village, is marvelous.

It will be seen from the foregoing that the experience in Massachusetts indicates that *Crenothrix* will not multiply in ground water, except

under special conditions which favor its growth, and these conditions appear to be produced when water is imperfectly filtered by continuous natural filtration. In view of this experience I have no hesitation in recommending ground water supplies in Massachusetts, where the conditions are favorable for obtaining a good supply, as being much less likely to cause trouble from the growth and decay of organisms than surface water supplies. The experience at Berlin and some other European cities should not, however, be lost sight of.

I am very much interested in Mr Rafter's paper, as it is an extremely valuable addition to our knowledge upon a subject concerning which our present knowledge is very limited.

Upon certain points we now have fairly definite information, as for instance the case already mentioned in this discussion, of *Crenothrix* developing in certain imperfectly filtered waters. We also know that ground water stored in an open reservoir will develop abundant growths of algæ, and that a portion of the inorganic nitrogen will be taken up by living organisms to the serious detriment of the quality of the water. It is further known that pure ground water stored in the dark will be practically free from these organisms and that its chemical constituents will remain unchanged.

When the case of the open storage reservoir is presented, it must be admitted that we have no definite knowledge based upon experience or well proved by scientific investigation which permits us to predict with a large degree of certainty the biological results to follow the construction of a storage reservoir, and we are equally unable to suggest a practical and sure remedy in cases where an existing reservoir is affected by growths of algæ.

Very large sums of money have been spent in the construction and improvement of reservoirs to prevent the growth of algæ, and in making these improvements theory has been the chief guide rather than scientific knowledge or experience.

This condition of affairs ought not to continue. The interests involved are large enough to warrant sufficient appropriations to keep a considerable force of biologists, chemists and engineers constantly employed on the solution of the problems presented.

Every isolated observation carefully made adds to the general fund of knowledge, but rapid advance cannot be expected without continuous observation.

I have here three diagrams, Plates LXXXVII, LXXXVIII, LXXXIX, prepared for another occasion, but which may be of interest in this discussion. These diagrams have been made from the results of monthly chemical analyses from June, 1887, to June, 1889, of the waters of two artificial storage reservoirs and a natural pond, all of them subject to abundant growths of algæ at certain seasons of the year. The particular feature of the chemical analysis indicated by the

diagrams is the albuminoid ammonia, which may be considered in these cases as indicating approximately the amount of algæ in the water.

It will be observed that the first two diagrams exhibit the same general features: namely, a very marked rise in the amount of albuminoid ammonia between June and November, culminating in August or September. In addition to the two reservoirs to which these diagrams relate there is another not represented which is affected by the same summer trouble. With regard to the extent of the trouble, it may be said that it is so serious that the abandonment of reservoirs and mains, costing upwards of $1 000 000 has been seriously considered. In one case the water has been affected every year during the thirteen years that the works have been in operation. The immediate cause of the bad taste and odor of these waters is the presence of abundant growths of algæ of the blue-green group. The reason why these algæ are found so abundantly in these particular reservoirs is not apparent. In one case, at least, it cannot be attributed to the shallowness of the reservoir or to the pollution either artificially or naturally of the water entering the reservoir.

The third diagram shows very different characteristics from the others. In this case there is but one very marked rise in the amount of albuminoid ammonia in the two years, and this occurs in the spring of 1889. In June, 1887, the amount of albuminoid ammonia was quite large, and much larger than the following month, so it seems probable that the growth was abundant in the spring of 1887, and was disappearing when the examination of the water of the pond was begun. There was no marked rise in the spring of 1888. The trouble in this case is due to the growth of a minute plant which is entirely different from the blue-green algæ found in the other cases referred to. At the time when the trouble culminated this spring, the water was so filled with these organisms that it was entirely unfit for use.

The advantages of circulation have been referred to in this discussion, and I will say a few words about it.

The Town of Brookline, Mass., has in connection with its low service supply an open reservoir supplied by pumping from a filter gallery. It was found that water of satisfactory quality when pumped would become unsatisfactory when stored in the open reservoir, and that the more the water was made to circulate by the addition of fresh water from the filter gallery the worse it became. The rule was therefore adopted to pump sixteen hours per day at a rate but slightly in excess of the consumption during the same time, and to allow only the slight difference in quality between the water pumped and the consumption to go into or be drawn from the reservoir; moreover, circulation is avoided as far as possible by drawing water from the reservoir at the place where it enters. A special experiment was made by Mr. F. F. Forbes, the superintendent of these works, to determine the effect of

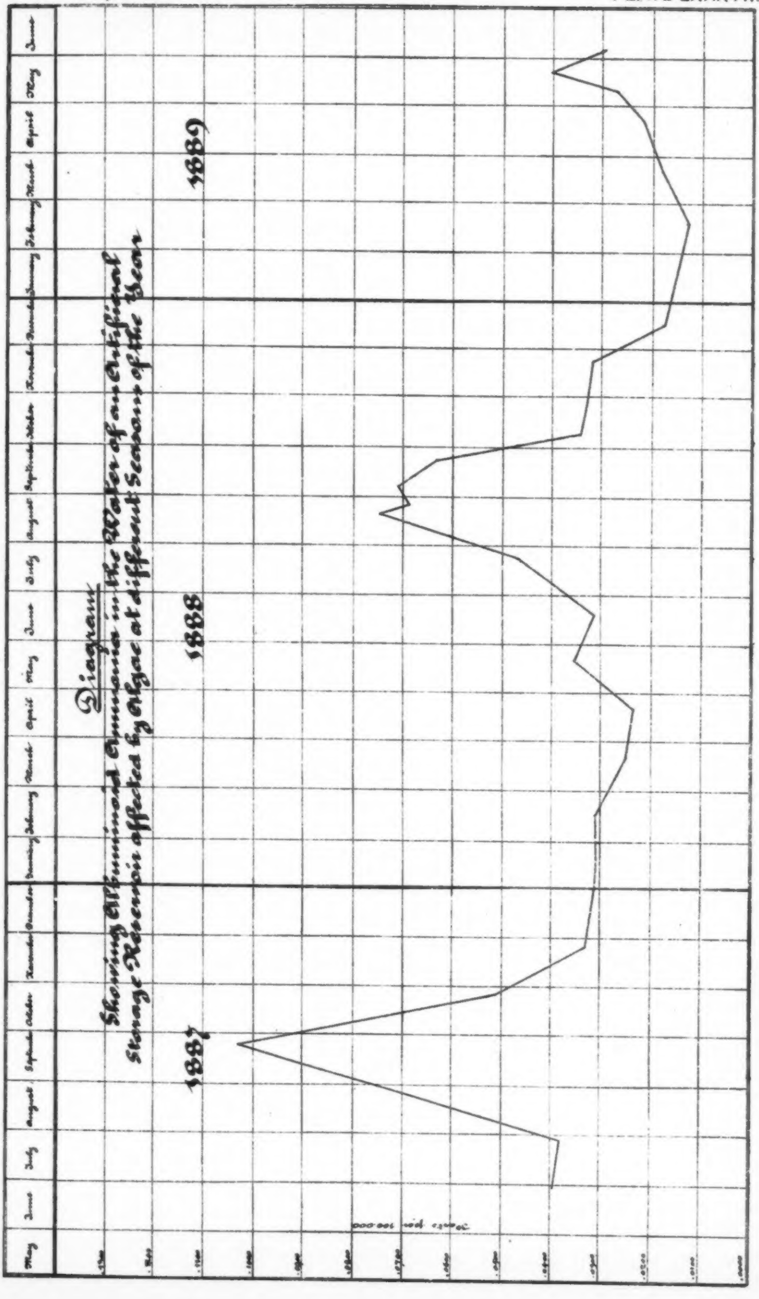


Diagram
Showing Albuminoid Ammonia in the Water of an Artificial Storage Reservoir affected by Algae at different Seasons of the Year
1887
1888
1889

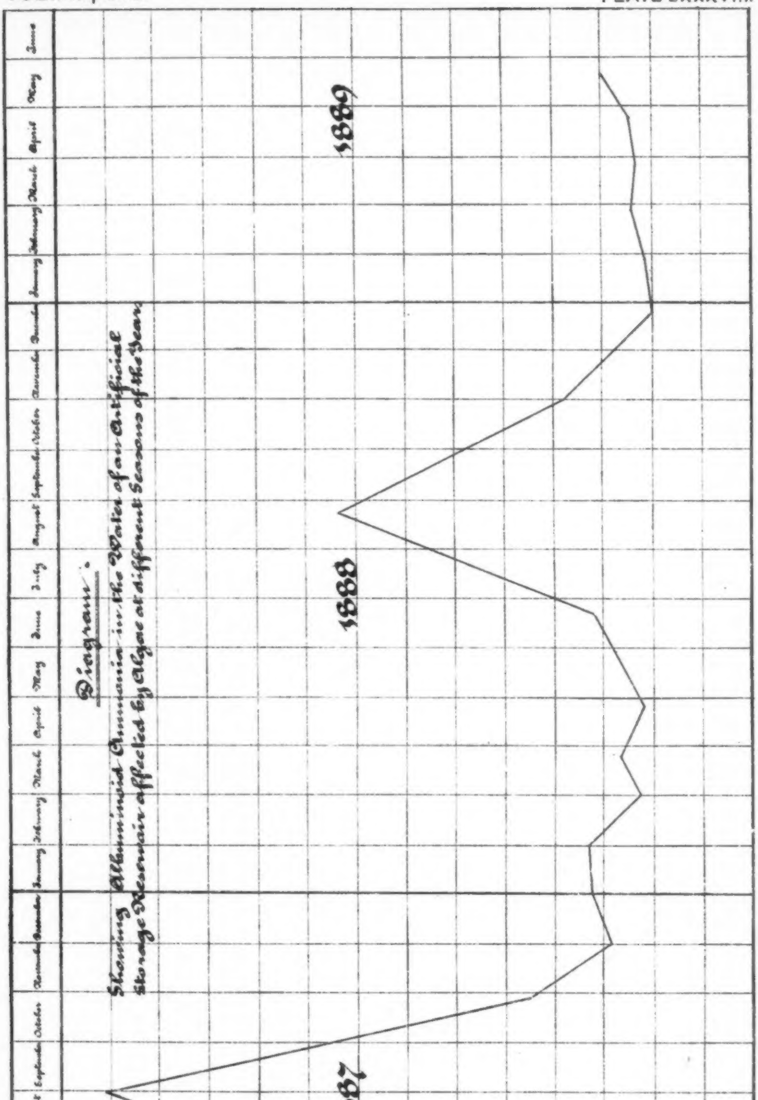


Diagram.
Showing Albuminoid Ammonia in the Water of an Artificial Storage Reservoir affected by Algae at different Seasons of the Year
1887
1888
1889
Parts per 100.000

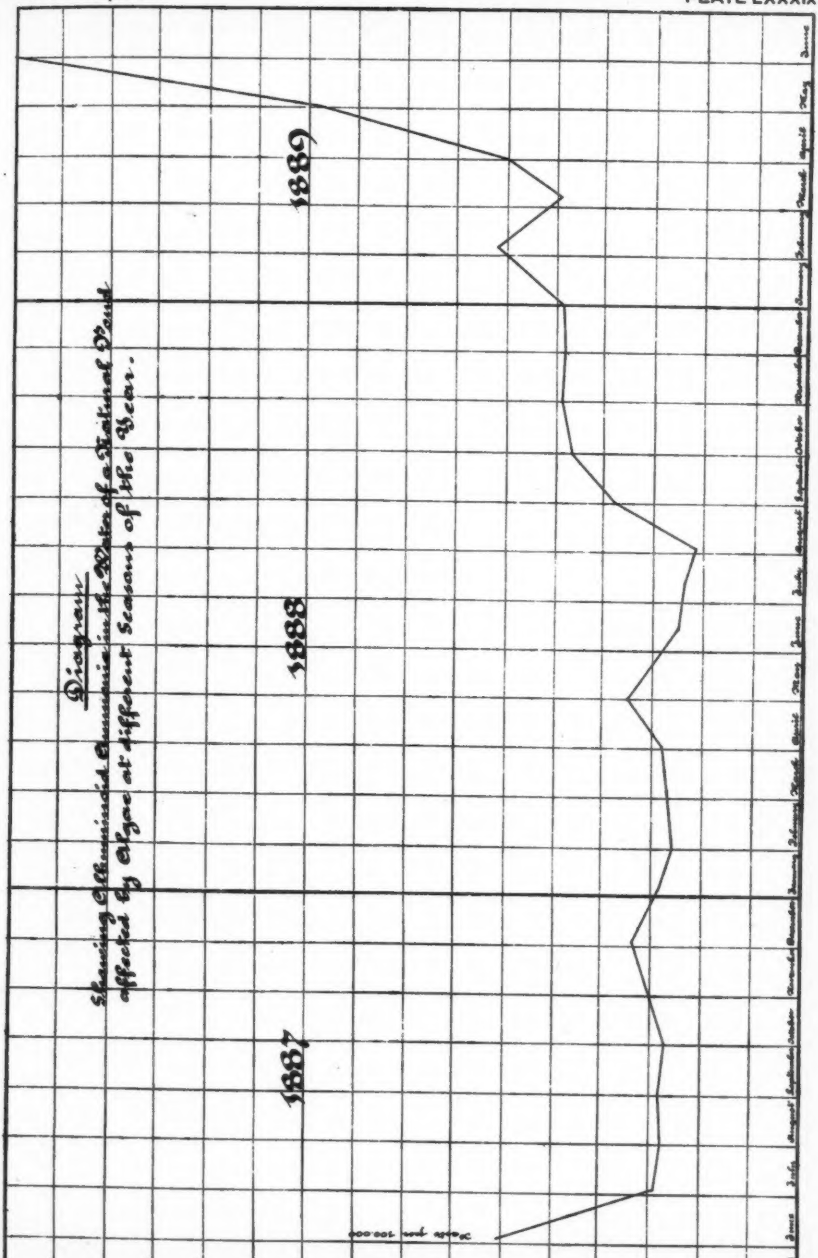


Diagram
Showing Albuminoid Ammonia in the Water of a Natural Pond
affected by Algae at different Seasons of the Year.
1887
1888
1889

adding fresh water. He first found that the water in the reservoir in its normal condition contained a certain number of organisms in a given quantity of water. The reservoir was next emptied, thoroughly cleaned, and filled with fresh water. The organisms, at first few, multiplied so rapidly that at the end of a week the water contained as many as before the reservoir was cleaned, and at the end of about three weeks it contained four or five times this number. After this time the number of organisms gradually reduced to the original number, but more than doubled when the reservoir was again partially drawn down and fresh water was added. Connected with these works is an iron tank supplying the high districts, having a diameter somewhat exceeding its height. This is said to have given more trouble than the large reservoir, and the superintendent attributed it to the fact that on account of its small size the water was necessarily changed in it quite frequently. The tank is now covered with a roof which excludes the light, and the water in it does not deteriorate. I do not wish these statements to be considered as an argument against the circulation of water in a reservoir under all conditions; but the facts stated show that circulation caused by the addition of fresh water containing food for the algæ will not prevent their growth.

Mr. Fteley.—It has been said that some very interesting experiments have been made in Massachussetts. It would be highly interesting to hear from Mr. Stearns the details of the work in which he has been engaged.

Mr. Stearns.—Three years ago an act was passed in Massachusetts, entitled "An Act to Protect the Purity of Inland Waters," and its execution was intrusted to the State Board of Health. The text of this act was published in the Transactions of this Society for January, 1888.

The primary object of this act was to regulate the disposal of sewage, so that the rivers and other inland waters should not be unnecessarily polluted. It is one of its requirements, however, that all cities and towns proposing to introduce a system of water supply or sewerage should submit their plans to the State Board of Health for its advice. It soon became apparent, from an examination of existing literature, that but little exact knowledge existed concerning the purification of sewage; and that there was much to be learned, locally and otherwise, with reference to the water supplies and inland waters of the State. For the next year an appropriation of $30 000 was asked for, with the view of establishing an experimental station for the purification of sewage, and of making a thorough examination of the water supplies of the State. This request and subsequent ones for $25 000 per year have been granted by the Legislature. As the subject of sewage disposal is not now under discussion, I will only say with reference to it that an experimental station was established at Lawrence, Mass., where experi-

ments upon sewage purification have been carried on for the past eighteen months under the supervision of Hiram F. Mills, C. E., member of the Massachussetts Board of Health. These experiments will probably add more to our scientific and practical knowledge of sewage disposal than any experiments which have ever been made.

Monthly examinations of all of the water supplies and many of the rivers of Massachussetts were begun in June, 1887, and these examinations of many supplies are still being made. The examinations are chiefly chemical and microscopical, though a few bacterial examinations have also been made. From this work much has been learned that will be of practical value in selecting sources and designing systems of water supply; but it is evident that there is an opportunity for a great deal of research in these matters before we shall know how to prevent bad tastes and odors in water stored in an open reservoir or pond.

The force engaged upon the examination of water supplies consists of six chemists, three engineers, and two biologists. A portion of this force, however, has other duties to attend to in connection with the work of the Board.

With regard to the importance of more information regarding the means of preventing the growth of algæ in water supplies, it may be said that out of about one hundred and thirty water supplies in Massachusetts, ten are seriously and almost chronically affected by bad tastes and odors due to such growths. Many other supplies are seriously affected at times. I have recently visited a pond in Massachusetts, which, fortunately, has not been used for a water supply, that has been afflicted for two years with an abundant growth of a dark jelly-like substance. This, at times, generally in warm weather, rises to the surface in patches as much as 10 feet in diameter and 2 inches thick, and is driven by the wind to the shore, where it accumulates in large quantities, and when heated by the sun is very offensive. The mass is composed chiefly of bacteria imbedded in jelly, but it also contains some algæ. I have mentioned this case, because it is apparently worse than any which have occurred in connection with water supplies, but I see no reason why the same trouble may not occur in any body of surface water used for a water supply. A possibility of this kind furnishes us with an additional incentive to acquire information upon these matters, which will enable us to undertake preventive or remedial measures with a knowledge of the results to be obtained.

Mr. Geo. W. Rafter.—In closing the discussion of the paper on fresh water algæ I may state that it was a matter of grave doubt with me as to whether the subject would be of interest to the Society, and I need hardly say that I am not only surprised at the interest manifested, but I am exceedingly grateful to the gentlemen who have, by taking part in the discussion, assisted very materially in the hammering process

which every idea must undergo before it attains the fixed form indicating complete evolution.

The remarks of Professor Leeds at the meeting of May 1st may first claim attention. Reference is made to the report of Dr. John Torrey upon the offensive condition of the Croton water in the summer of 1859, and Professor Leeds very justly ascribes great credit to Dr. Torrey for the clear views which he even at that relatively early day presented. I agree with Professor Leeds as to the value of Dr. Torrey's report. It has been in my possession for a number of years; and I have been in the habit of consulting it as the original American classic on this subject. (See Appendix, page 555.) Nevertheless, I cannot but conclude that we have made great advances in our knowledge since Dr. Torrey wrote in 1859. At that time there was absolutely no American work on cryptogamic botany in existence, and even the great work of Rabenhorst which, by reason of being written in the universal Latin language, speedily became the handy reference book of all students of the cryptogams, did not appear until five years later. In England, Hassall's work, a new edition of which had appeared two years previous, was then about the only available book,* and the American student of that day could hardly find a more satisfactory reference. If we consider, therefore, the paucity of the literature which even an eminent botanist like Dr. Torrey had at hand, it is not to be wondered at that he hardly compassed the whole question, and, at the same time, the clearness of his views in many particulars easily leads to the conclusion that he had given the subject as an independent proposition very careful consideration. Professor Leeds, however, without actually saying so, unintentionally leaves the impression that Dr. Torrey, at that time, clearly affirmed the oil globule as a cause of objectionable tastes and odors, but a cursory reading of what Dr. Torrey said will remove all misapprehension on this point.

Professor Leeds is right in assuming that the starch experimented with was not chemically pure, that it was in short ordinary commercial starch. As the result, however, of some farther trial I arrive at the conclusion that the failure of chemically pure starch to give the odors in question is a matter of little or no significance. We are not dealing with the starch as a finished chemical product, but rather with it as in process of formation; we are dealing in short with the chemical processes in the plant rather than with such processes in the laboratory. The formation of starch in the growing plant is a matter about which nothing positive is yet known farther than this, that while the completed starch grain is by itself a pure carbo-hydrate, it is, nevertheless, in some way a product of the nitrogenous protoplasm, and so intimately related to it that we do no violence to the present state of knowledge by taking the starch grain in process of formation as one of the typical causes of the class of

* "A History of the British Fresh Water Algæ," by Arthur H. Hassall, M.D., 1857.

difficulties under consideration. The question is one with more than theoretical interest, as will be sufficiently shown by considering that the production of starch in all the chlorophylaceous algæ is entirely dependent upon the quantity of light which these little plants receive. All of the free-floating forms are from a variety of causes, as, for instance, changes of temperature, cessation of the production of gas by the plants themselves, etc., quite susceptible to changes in specific gravity, and will, therefore, at different times, occupy different levels in the water. In light of less than a certain degree of intensity the starch is not formed, the protoplasmic matter, which, with sufficient intensity of light, would go to the production of starch, remains protoplasmic. Again, if algæ in which starch is fully formed are placed in the dark, or in light of less than the starch-producing degree of intensity, the starch already formed will disappear, such changes taking place as restore the starch material to its original state. On being again brought into strong light the starch will reform, and by treatment in a suitable culture cell, all these transformations can, under proper gradation of light, be easily observed for a considerable length of time.

The application to be made of these observations is in relation to the changes in intensity of the light which will exist at different depths in any given body of water, and consequently in relation to the varying quality of the water itself at different depths. In this connection it is important to clearly understand that the production of chlorophyl and starch is very intimately related to the chemical composition of water, and that if such conditions obtain as preclude the continuance of their formation marked changes in chemical composition will immediately result.

Professor Nichols made in 1879 and 1880 a series of chemical analyses of Mystic lake water at different depths, and the results show clearly the correctness of the foregoing proposition. (See Appendix, page 557.) They show farther that at both top and bottom the free ammonias attained their maxima in February, when the cold weather of winter had partially arrested the vital processes of the algæ, and that the free ammonia was lowest at that time of the year when the algæ are known to be in their most active state, namely, in October. We have only to consider the important office of the free ammonia as food for living plants to appreciate the value of these little plants as purifying agents.

Changes in quality of light as depth increases may result from two causes, or rather from a combination of two causes. There will always be either an increased opacity of the water itself, due to increase of coloring matter as the depth increases; or where the coloring matter is constant at all depths, the decrease in intensity of light will be in accordance with the general law that intensity decreases geometrically as distance increases arithmetically. By way of illustration, we may assume the opacity of a given body of water to be such as to cut off $\frac{1}{20}$ of the total intensity of light at the depth of one foot. We have then after

passing through one foot of such water $\frac{19}{20}$ of the original intensity at the surface. In passing through the second foot the light will again lose $\frac{1}{20}$ of the total quantity of light entering the second foot or $\frac{1}{20}$ of $\frac{19}{20}$. At the depth of two feet the total intensity is therefore $\frac{361}{400}$ of the original intensity, and so on for any depth whatever.

The remarks of Mr. Stearns bring into the discussion very forcibly the whole question of the relation of chemical constituents to organized life, and indeed open up a whole field of new discussion. Before entering upon this discussion of the subject we need to define clearly the relation of the plant life to the ammonias both free and albuminoid; thus, for instance, albuminoid ammonia in its relations to matters biological is the measure of the algal life present in the water at the time of making an analysis, while free ammonia is in the same way a measure of past algal life, of that which has existed at some former period, but which has decayed and passed beyond the state of organized life. If we make, therefore, a quantitative determination of the amorphous sediment present in all waters the results will probably run in some clear relation to the free ammonia. In the same way a quantitative determination of the life itself will show equally a clear relation to the albuminoid ammonia. This has been clearly apparent to me for some time, but in the absence of any method of making the quantitative biological determinations actual results could not be arrived at. Professor Wm. T. Sedgwick, of the Massachusetts Institute of Technology, has, however, recently published a method of making such determinations,* and improvements in that method by the present writer leave nothing to be desired. The quantity of life present in any given sample may now be determined with a considerable degree of precision, and if we gain nothing else by such examinations, the clearing up of our ideas as to just what the ammonias mean biologically is still a great advance.

The ammonia process has by common consent come to be regarded as the most satisfactory chemical determination of the sanitary value of potable water that has yet been devised, and its authors, working largely upon the polluted waters of London wells, fixed upon 0.015 parts in 100 000 as the highest permissible limit of albuminoid ammonia; but with a clear idea of just what the albuminoid ammonia means, this arbitrary standard is shown to be utterly misleading. Professor Wanklyn states in the preface to his last edition (October, 1888) that he has used the ammonia process for twenty-one years, and it is indeed astonishing that the biological side of the question has never presented itself to his mind, but a perusal of his last edition leads inevitably to the conclusion that it never has. Working largely on the polluted ground waters in the vicinity of London, he still affirms the original

* Paper on Recent Progress in Biological Water Analysis, in the Journal of the New England Water Works Association, September, 1889, by Wm. T. Sedgwick, Associate Professor of Biology, Massachusetts Institute of Technology.

standard the same as twenty years ago. This may be considered as merely the natural effect upon an individual of the environment, for equally eminent chemists differently situated have long since learned the inapplicability of this arbitrary limit, as a universal standard. Thus Professor Leeds, at the outset of his elaborate study of the Philadelphia water supply, found it necessary to prepare new standards, and as the result of several hundred analyses prepared the following, which he has designated as sanitary standards:*

Constituents Expressed in Parts per 100 000.	General Standard of Purity (for River Waters in the United States—Highest upper limits.)
Free ammonia	0.001 — 0.012
Albuminoid ammonia	0.01 — 0.028
Required oxygen as determined by permanganate	0.35 — 0.5
Required oxygen as determined by silver	?
Nitrous acid (HNO_2)	0.0001 — 0.001
Nitric acid (HNO_3)	0.35 — 0.5
Chlorine	0.3 — 1.0
Hardness	5.0 for soft—15.0 for hard.
Total solids	15 — 20
Oxygen dissolved per liter	?

Professor Leeds states in the same connection two principles which must be kept in mind in determining standards of purity, thus:

First.—The results of chemical analysis must confirm and explain the facts gathered by personal inspection.

Second.—There must be established for every source of water supply, whether cisterns, spring, well, lake or river, a standard of purity according to which the quality of the water must be judged.

That these two principles are literally true is becoming more and more apparent every day, and their clear enunciation in 1883 was a step in advance in our knowledge of the real significance of chemical analysis.

Professor Drown, working in the same direction, has shown that brown-colored waters require to be judged by a different standard from that applied to colorless waters.†

The field has thus considerably widened until at the present time we may affirm that a complete study of any given water supply should include *four* distinct examinations, which for present purposes are sufficiently indicated by the outline table on the opposite page.

* See a Preliminary Report of a Chemical Investigationin to the Present and Proposed Future Water Supply of Philadelphia, by Albert R. Leeds, Ph.D., in Annual Report of the Philadelphia Water Department for 1883, page 243.

† The Odor and Color of Surface Waters, by Thomas M. Drown, Professor of Analytical Chemistry in the Massachusetts Institute of Technology, etc., in the Journal of the New England Water Works Association for March, 1888, pages 2-29.

Environmental (1).		Including detailed statement of Topographical and Geological Conditions of Drainage Area, together with Observations on Extent and Character of Population and Industries of Region as Special Sources of Pollution, with study of Normal Samples by (2), (3) and (4).	
Physical (2).		Depth from which Sample is Taken.	
		Temperature.	
		Specific Gravity at the Actual Depth and Temperature.	
		Color and Odor.	
		Intensity of Light at Actual Depth.	
Chemical (3).		Albuminoid Ammonia.	
		Free Ammonia.	
		Nitrous Acid.	
		Nitric Acid.	
		Oxygen Required.	
		Chlorine.	
		Hardness.	
		Total Solids.	
Biological (4).	Plants:	Chlorophyceæ.	
		Cyamophyceæ.	
		Schizomycetes.	
		Animals.	
		Amorphous Organic Matter.	

The last column represents the formless material which makes a portion of the sediment, and for which as the most appropriate designation I have used the term *amorphous matter*. Zoogloæ has also been proposed, but I may suggest in reference to this term that it has already acquired a definite meaning; namely, a colony of putrefactive bacteria, imbedded in a mass of jelly. The amorphous matter, as representing organic matter, which has passed so far into the state of decay as to be without definite form, may or may not comply with the strict terms of this definition. In many cases it certainly does not, and in all such the application of zoogloæ would be inappropriate. At other times it would undoubtedly be scientific to say amorphous matter in the zoogloæ state; but the application of zoogloæ as a generic term for the phenomenon in question can hardly be considered permissible.

With a study of a series of samples carried out as here outlined, we could reasonably expect to gain information relative to a given water supply quite up to the present state of knowledge.

Professor Waller's discussion presents facts of very great interest. Thus, his observations in 1881, at the time referred to by Mr. Hyatt, that filtering the Croton water removed all of the free ammonia and nearly all the albuminoid ammonia was a great incentive to a clearing of somewhat misty views on my part, and I hope it may be equally prolific to others. Professor Waller is, however, mistaken in supposing that I absolutely deny the presence of oil in the *Cyanaphyceæ*. If he will kindly re-read the original paper he will see that the statement really is that starch and oil are not the most important characteristics, but that in the plants in question the mucilaginous jelly is a more important feature.

The last paragraph of Professor Waller's discussion demands momentary consideration. Thus, in regard to the ammonias, nitrites and nitrates, I may say that while these substances usually exist in potable waters in such minute quantities as to be, if just added, entirely harmless, nevertheless, the ammonias and nitrites, as representing organic matter in a state of change, are very intimately connected with any unwholesome conditions of the water. In the nitrates we may consider the organic nitrogen completely reduced to the inorganic form, and hence incapable of further change. In further extension of this part of the discussion I adopt the view of Professor Drown, that if the algæ take up and remove from the water organic substances capable of undergoing change, they are probably doing a good work, but we should not lose sight of the fact already referred to in a previous part of the discussion, that nitrates may themselves, under favorable conditions, become food for algæ and the direct cause of such growths in waters, which would be free from them if the nitrates were absent.

Since writing the original paper I have considerably extended my knowledge of the jelly-producing capacity of the diatoms, and as this is

undoubtedly a very important phase of the question, I will briefly record the results here.

On May 10th, 1889, I observed the sides of the masonry of inlet well and the rip-rap at Mt. Hope reservoir of the Rochester supply to be covered with a filamentous growth which, at that date, had a length of several inches. On examination it turned out to be principally two diatoms, *Encyonema* and *Cocconema.* The former is distinguished by a cymbiform frustule, arranged in longitudinal series within tubular filaments. The latter has a stipitate supporting frond with the frustule at the extreme end. Both were present in quantity, and the sub-membraneous filament of the one and the supporting frond of the other together made a considerable mass of nitrogenous material. On May 22nd and 23rd, reports were brought to the Water Works Department that Hemlock lake water, as delivered to the consumers, had suddenly acquired a strong fishy taste and odor, and further investigation showed that the moving filaments and fronds of two weeks before had contracted to the depth of one-half inch, and were only apparent as masses of compact jelly. Study at Rush reservoir and at Hemlock lake itself revealed the fact that a similar growth was present at both places, and after going over the lake and both reservoirs quite carefully, I estimated the total quantity of this compact jelly as at least 2 000 cubic yards. A careful study with the microscope showed that while the greater portion of this was *Encyonema* and *Cocconema*, there were also present in the jelly-mass *Fragillaria* (two species), *Cyclotella*, *Stephanodiscus*, *Asterionella*, *Synedra*, *Gomphonema* and *Pleurosigma Spencerii.*

Tests were also made of samples of water from different depths and upon the jelly itself, similar to those already described in the original paper in the account of *Volvox globator*, and no doubt remained as to the connection of the jelly with the fishy taste and odor.

The persistency of the fishy odor was well illustrated by accidentally spilling some hot water containing the jelly over my clothing, whereupon I carried about the smell of a fish-market for several hours.

On May 28th and 29th, very heavy wind prevailed for more than thirty-six hours, and at its cessation not a single vestige of the jelly-mass could be found at either lake or reservoirs. When, however, a few gallons of water from the lake itself were concentrated by filtering, into about 100 cc., it was found to contain a white, slightly flocculent sediment, whereas the same treatment previous to the heavy wind yielded nothing of the sort. Similar treatment of samples from the city distribution yielded a brown-colored sediment, evidently the same as that from the lake except in the matter of color. Samples from different depths in both reservoirs showed the same as that from the lake, *i. e.*, white-colored sediment, while those taken from the main conduit just before entering reservoirs, showed brown-colored, same as from city distribution, thus clearly showing the reducing effect upon the distributed jelly of agitation while passing through the mains.

This study was of further interest by reason of the light it threw upon the cases which have been cited by Mr. Fteley of tastes and odors apparently unaccompanied by the presence of any plant life. Had no knowledge been gained of the seat of the difficulty previous to the heavy winds of May 28th and 29th, there would have been absolutely no chain of circumstances by which to connect the fishy taste and odor with the rankly growing diatoms of May 10th, and I have no doubt but that an equally certain physical cause will be found in every such case if all the attendant circumstances can be got at.

On June 1st, heavy rains fell (+ 3 inches), and the consequent inflow to Hemlock lake was sufficient to immediately reduce the intensity of the fishy taste and odor by quite an appreciable amount, and finally about June 25th, it was found to have entirely disappeared.

Daily examinations were made by two observers during the whole time of the continuance of the fishy taste and odor, and one fact of considerable significance was clearly brought out, namely, the marked absence of all kinds of animal life. Hemlock lake water at the beginning of June usually abounds in certain rotifers and entomostracans, but at this time, except on the surface, they were almost entirely absent. The plant life was also quite low, less than usually observed, and it is matter of regret that no quantitative determinations were made. Had such a series been carried on both before, during and after the fishy taste and odor, we would be able to answer definitely many practical questions of which at present the solution can only be inferred.

What may be termed accidental experience during the last summer leads to the conclusion that this matter of the formation of jelly on a large scale by the diatoms is very common. Thus at Batavia, New York, in August, I found an area of several acres over which water enough flowed from a series of springs issuing from the base of a hill, to keep the ground partly free of vegetation. This area had growing upon it masses of diatomaceous jelly similar to that found at Hemlock lake and the Rochester reservoirs earlier in the season, and in sufficient quantity, if one cared to gather it, to fill many bushels.

Later in the season (October 15th) a similar growth was found at Canadice lake, different, however, in this, that while all the other jellys seen this year have been characterized by absence of animal life in the jelly itself, this Canadice specimen contained vast numbers of green infusoria, which had comfortably established themselves throughout all parts of the jelly.

The origin of the jelly mass is still involved in considerable obscurity, nothing like a complete explanation having yet been made. It is probable, however, that so far as relates to the diatoms the general statement of the Rev. William Smith, thirty-five years ago, that the *Diatomaceæ*, while possessing special features of their own are intimately allied to the other orders of the *Protophyta*, especially to the *Nostocaceæ*, in

the tendency shown by several genera to surround their frustules with frondose masses of mucus, is essentially correct.*

Among recent publications, the most complete observations on the origin of the gelatinous envelope of diatoms are those of Professor Hamilton L. Smith,† and his conclusion is probably true that the exceptional development of the enlarged sporangium, which is the usual accompaniment of extensive diatomaceous jelly masses, is merely a phase of reproduction, by which, after a long series of vegetative generations by division, there is in effect a rejuvenescence by conjugation, accompanied by a material increase in the size of the resulting primary generation over that of the parent cells, which uniting by conjugation, formed a new sporangium and its investing mucilaginous gelatine. From whence it happens that, after a long series of vegetative generations by division, the time arrives, in the due course of the life history, for reproduction by conjugation. If at such time the conditions are favorable for this taking place on a large scale, we will have in certain species in which this tendency to sporangial development is particularly pronounced, the formation of considerable masses of the mucilaginous gelatine. The basis of this view is stated by Professor Smith to have been first advanced by Braum in his "Rejuvenescence of Nature," in 1851. It is also held as true by McDonald and Dr. Greville, as cited by Professor Smith, and this consensus of distinguished opinion is entitled to the most careful attention.

The foregoing is merely a brief outline of a very small portion of Professor Smith's paper, and for the complete detail I must refer those sufficiently interested to pursue the matter further to the paper itself.

This question of the excretion of a protoplasmic gelatine either just before or during impregnation has been studied by A. Dodel-Port, who advances interesting views in a recent paper.‡

According to his view certain algæ always excrete protoplasm during the coalescence of the conjugating cells. In the higher types it would appear as if this excretion, which is usually from the female cell before impregnation, had simply for its object the formation of a nidus for the reception of the male fertilizing agent.

Professors Sachs and De Barry have both studied the origin and composition of the mucilaginous gelatine. De Barry, more particularly in relation to its connection with the development of the fungi and bacteria, while Sachs' studies are general in their character, including the vegetable mucilage, not only of phenogamous plants, but also of algæ, fungi and bacteria.

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*A Synopsis of the British *Diatomaceæ*, by the Rev. William Smith. London, 1853-56. Introduction to Vol. II, page 21.

† Paper—A Contribution to the Life History of the *Diatomaceæ*, Part II, by Professor H. L. Smith, in the Proceedings of the American Society of Microscopists, 1887, pages 126-167.

‡ Biologische Fragmente, by A. Dodel-Port, Part II. Cassel and Berlin, 1885. Also Bot. Centralbl., XXIV (1885), page 132. Also J. Roy, Micr. Soc., 1886, page 278.

Sach's latest views may be summarized as follows :* The change which results in the production of the gelatine may, under certain circumstances, occur as a diseased condition, while in other cases it appears as a normal change, serving definite purposes of life ; and in the economy of the algæ and many of the fungi this change plays so important a part that the form and mode of life of such plants are to a certain extent determined by it.

De Barry's observations on the development and life-history of the fungi and bacteria lead to essentially the same conclusions.†

The views of both Sachs and De Barry are too elaborate to be gone into at length, and my present object in referring to them at all is merely to call attention to their works as throwing considerable light on the general subject.

A paper of this sort ought to include some discussion of the question of remedy. I do not, however, give that part of the subject special consideration at this time, chiefly because I am not fully prepared to make such thorough discussion as it demands. Many of the hints of the gentlemen who have taken part in the present discussion are valuable, and in some cases fully cover the ground, but for the more serious difficulties of the kind under consideration my present view is that sand filtration is the most efficient remedy that can be devised.

Mr. Fteley has suggested the appointment of a special committee of the Society for the purpose of corresponding with various water boards in order to ascertain whether it would be possible to join in a systematic study of these matters under concerted action. Such a study, if intelligently directed, would be productive of practical results of great value, and is clearly worthy the Society's attention.

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*Lectures on the Physiology of Plants, by Julius Von Sachs, Translated by Ward. Clarendon Press, 1887, pages 89–90.

†Comparative Morphology and Biology of the Fungi, Mycetozoa and Bacteria, by A. De Barry, Professor in the University of Strassburg, Translated by Garnsey. Clarendon Press, 1887, pages 9–12. 456–460.

APPENDIX.

The following is Dr. Torrey's Report:

New York, August 25th, 1859.

To Myndert Van Schaick, Thomas B. Tappan and A. W. Craven, Esqs., Croton Aqueduct Board:

Gentlemen,—At your request, and in company with Dr. Chilton, I visited Croton lake on the 18th inst., for the purpose of ascertaining, if possible, the cause of the disagreeable quality observed in the water distributed through the city, and which had been the subject of serious and general complaint for a week or two previous. On driving down the hill that leads to the dam, and before reaching the lake, we noticed the same musty odor that characterized the water of the hydrants in the city. Mr. Adamson, the gate-keeper, afforded us every facility in prosecuting our researches, and piloted us to various places on the lake. He brought to our notice a bright green substance that had appeared within the last twenty-four hours in the water near the dam. This material had been driven by a favorable wind, to the outlet of the lake, where it accumulated so as to form a considerable stratum in quiet recesses near the shore. The water at the time was very low, only a small portion flowing over the dam. A quantity of the water, quite thick with the green material, had been reserved for our examination, and we had collected some of it ourselves from the surface of the lake. We soon became convinced that this unusual ingredient was the cause of the peculiar taste and odor of the water. After examining the character of the larger aquatic plants that grew abundantly in the western portion of the lake, and taking new samples of the water from various places, we returned to the city, and I commenced my examination immediately, before any obvious change had taken place in the properties of the water.

On placing a portion of it under a microscope, it was found to be filled with little straight filaments, which were composed of oblong roundish cells, in a single row like a string of beads, and in no case did I find these threads to be branching. Most of the cells were about one-third longer than broad, and were filled with a bright green substance, composed of irregular grains. This matter was proved to be chlorophyl, or the green coloring substance of leaves. Interposed here and there in the bead-like filaments were two other kinds of cells, the one perfectly spherical, three times the size of the oblong ones, and filled with green spores or seeds, by which the plant is reproduced ; the other also spherical, but much smaller, and containing a yellowish fluid, but no green grains. The function of the latter is unknown to me.

There were only a few other kinds of microscopic plants in the water, nearly all of which belong to a particular tribe, called by botanists *Desmideæ*. In ordinary seasons these constitute the chief vegetable forms existing in the Croton. The number of living animalculæ was also unusually small. In repeated instances, on concentrating the water of the city hydrants, at the time that it exhibited the offensive properties complained of, I found its characteristics precisely those of the water taken at the dam. The day after collecting the sample of bright green water at the lake, I submitted it again to the microscope, and was surprised to find that all the beaded filaments had disappeared, but there was floating in the liquid, in a separate state, the larger spherical green cells and the smaller yellowish ones. I had noticed in my first examination several of the filaments break up by the successive bursting of the little

cells. Without doubt the myriads of little plants obtained in my sample had disappeared in the same way. I have since repeatedly seen the filament break up in water taken from the hydrants. They undergo dissolution much more quickly when they are confined in a bottle. After the rupture of the cells the water retains its green color for some time, but it finally becomes bleached from the decomposition of chlorophyl. The bright green water from the Croton dam became quite viscid in less than twenty-four hours, and the following day it was somewhat putrid, and emitted a little ammonia, but not the least trace of sulphureted hydrogen. Most of the spherical cells just noticed remained entire, but the green ones had evidently matured their spores or seeds.

Before the green water had decomposed, I evaporated a portion to dryness, with great care, as as to avoid scorching it. Sulphuric ether agitated with the residue became of a lively grass green tint, but without materially diminishing the color of the mass. The solution after spontaneous evaporation of the ether left a thick brownish green matter, which was resoluble in ether and alcohol but not in water. It was doubtless one of the coloring bodies of chlorophyl. A portion of the green water was then boiled for some time; it lost its color and odor, and deposited brownish flakes on cooling. The water that distilled over contained the odorous principle in a concentrated form.

From this examination, and from the researches of Dr. Chilton, I think we are warranted in concluding that the recent offensive condition of the Croton water was owing to a rapid and abundant growth o m croscopic conferva-like plant, which abounds in a volatile, odorous principle, soluble to some extent in water.

We were extremely fortunate in making our visit to the lake at just the time when a favorable wind drove the little plant (floating by adhering bubbles of oxygen gas) to the outlet; thus bringing within the space of a cubic inch as many of the filaments as could be separated by filtration from a hogshead of the water a short distance from the dam.

I have not yet satisfied myself as to the origin of these little filaments; whether they are an entire plant, or once constituted a part of a more complex alga. They are more minute than in any true conferva known to me, being over 2500th to 2000th of an inch in diameter, and from a 50th to the 20th of an inch long. I strongly suspect that they are derived from a species of the genus *Nostoc* of botanists. The genus usually consists of a globular or oblong vesicle, from the size of a buckshot to the bigness of a plum, and filled with mucus which is loaded with minute, bead-like filaments. When the little bladders burst the contents escape, the mucus dissolves in the water, and the filaments are set free. Sometimes the waters of small lakes are filled with these small bladders of *Nostoc*. Since examining the water I have had no opportunity of revisiting the lake to verify my conjecture.

The question naturally arises, why this plant should have made its appearance in such quantities, and not have been noticed before? We can only reply that the case is not a singular one. Even in the higher order of plants, it is a common circumstance for a particular species to abound at one time, and then almost disappear for years; and in the lower vegetable tribes, especially those which inhabit the water, is this strikingly the case. The present summer has been unusually favorable to vegetation of all kinds; but what are the circumstances which have so remarkably multiplied this little alga I have not yet determined. It is a plant of short duration, and should it reappear another season, the probabilty is that it will not annoy us long. Very likely more or less of

it occurs every summer, but this is the first time that it has been offensively brought to our notice. Even when it was most abundant in the Croton, I do not believe that it communicated any unwholesome quality to the water. Its odor and taste were certainly unpleasant. While the unpleasant quality of the water continues, the ordinary filter will remove all the suspended matter, and a little fresh-burned vegetable charcoal or animal carbon, will take away the disagreeable smell and taste. A good method of using the charcoal is to heat some small pieces red hot, and, after quenching, inclose them in a little bag of gauze, and suspend them in the water. The charcoal should be renewed or reburned every day or two.

I am, gentlemen, very respectfully,

Your obedient servant,

(Signed) JOHN TORREY.

Professor Nichols' results so far as the ammonias and solids are concerned are embodied in the following table compiled from Report of Boston Water Board for 1880.

EXAMINATION of Mystic Lake Water by Professor Wm. Ripley Nichols (Results expressed in parts per 100 000):

DATE.	18 feet below surface.			50 to 78 feet below Surface.		
	Free Ammonia.	Albuminoid Ammonia.	Total Solids.	Free Ammonia.	Albuminoid Ammonia.	Total Solids.
October 18, 1879....	0.001	0.017	9.1	0.007	0.011	7.8
November 8, "	0.008	0.011	9.6	0.009	0.011	9.6
" 15, "	0.008	0.013	9.6	0.023	0.027	11.6
" 22, "	0.015	0.013	9.8	0.016	0.017	9.6
December 8, "	0.011	0.011	9.6	0.011	0.008	9.7
January 30, 1880...	0.031	0.016	11.1	0.027	0.016	11.8
February 6, " ...	0.035	0.019	11.1	0.080	0.019	11.8
" 17, " ...	0.041	0.016	10.8	0.075	0.017	11.5
March 6, " ..	0.040	0.016	10.4	0.040	0.015	9.5
April 5, " ...	0.040	0.013	10.2	0.041	0.013	10.2
" 14, " ...	0.040	0.016	10.4	0.043	0.017	10.3
May 3, " ...	....	....	....	0.048	0.019	10.1
" 12, " ...	0.032	0.016	10.5	0.064	0.016	10.3
" 24, " ...	0.040	0.016	10.6	0.064	0.015	10.5
June 3, " ...	0.036	0.017	10.3	0.049	0.016	10.6
Averages	0.027	0.015	10.2	0.040	0.016	10.3
Average at top during the same time, October, 1879-1880..				0.028	0.018	10 0
Average as drawn in Charleston during the same time..				0.019	0.015	9.8

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ON THE EFFECT OF A RAPIDLY INCREASING SUPPLY OF WATER TO A STREAM, ON THE FLOW BELOW THE POINT OF SUPPLY.

By JAMES B. FRANCIS, Past President Am. Soc. C. E.

WITH DISCUSSION.

In the Chezy formula for the flow of water in channels, viz.:

$V = C\sqrt{RI}$;

V = velocity in feet per second;

C = a co-efficient depending on the nature of the bed of the channel;

R = the hydraulic mean depth;

I = the slope or descent per foot in length.

In this case C and I may be taken as constant. The depth of the stream, and consequently the value of R, will increase with the quantity of water flowing, and by the formula, the velocity will increase as $\sqrt{R}$ increases.

The increased velocity due to the increased quantity will be first felt near the point of supply, and later in the stream below, and the effect will be that after a sufficient time the upper parts of the stream having

a greater velocity, will overtake the lower, combining with them and flowing on together, with a depth and velocity due to the greater quantity.

This effect will be most distinctly indicated in a stream having a rapid descent, and may be illustrated as follows:

In a stream having a rocky bed, for which $C = 50$, suppose the descent to be 1 foot in 100 feet, or $I = 0.01$; and suppose the section and wetted perimeter to be such that $R = 1$. Then, by the above formula,

$V = 5$ feet per second.

In 100 seconds from a given moment suppose the supply of water, and with it the depth of the stream, to be increased so that $R = 4$. Then the velocity will be increased to $5\sqrt{4} = 10$ feet per second.

Put $x =$ the time from the admission of the additional supply in which the increased current will overtake the original current, which has had a start of 100 seconds, $100 \times 5 + 5x = 10x$, from which we find $x = 100$ seconds.

The distance from the point of admission of the additional supply to the point where it will overtake the stream flowing 5 feet per second will be $100 \times 10 = 1\,000$ feet.

After the lapse of another 100 seconds, suppose another additional supply is admitted from the same source, increasing the depth so that $R = 9$, and the velocity becomes $5\sqrt{9} = 15$ feet per second.

For the time x from the admission of the second additional supply, when it will overtake the first additional supply, which has a start of 100 seconds, we have $100 \times 10 + 10x = 15x$, and $x = 200$ seconds.

The distance from the point of admission of the additional supply where it will overtake the preceding will be $200 \times 15 = 3\,000$ feet.

After the lapse of another 100 seconds, another additional supply is admitted, increasing the depth so that $R = 16$, and the velocity becomes $5\sqrt{16} = 20$ feet per second.

Similarly, the time x from the admission of the third additional supply when it will overtake the second additional supply, which has a start of 100 seconds, will be $100 \times 15 + 15x = 20x$; and $x = 300$ seconds, and the distance from the point of admission, where it will overtake the preceding, will be $300 \times 20 = 6\,000$ feet.

After the lapse of another 100 seconds, another additional supply is admitted, increasing the depth so that $R = 25$, and the velocity becomes $5\sqrt{25} = 25$ feet per second.

The time x when it will overtake the preceding will be $100 \times 20 + 20x = 25x$, and $x = 400$ seconds, and the distance from the point of admission where it will overtake the preceding will be $400 \times 25 = 10\,000$ feet.

If there is no further increase in the supply there will be no further increase in the value of R or in the velocity. If the supply diminishes the value of R will diminish also at the point of supply, but the effect of the preceding additions will not be reduced, even if the supply should entirely cease, and R become zero at the point of supply, and from the moment when the current due to the latest increase in the supply has overtaken the preceding currents, and of course become the head of the torrent, it will continue to flow down the channel with the same depth and velocity so long as the descent and form of the channel continue uniform, and for the same length of time that the maximum supply is continued, after which it will begin to be reduced in depth and velocity.

Instances of these effects in mountainous countries are not uncommon. A heavy local rain in an elevated region would obviously cause a rapidly-increasing flow in a channel in which the flow was previously small or even nothing, and as the flow increased, the effect would be as above described, and a torrent would appear suddenly at points below, where there had been no rain or even knowledge of its occurrence elsewhere.

There have also been several instances of the failure of reservoir dams, in which similar effects have been observed; notably in this country the Mill River Reservoir Dam, which failed in 1874, involving the loss of 143 lives, mainly due to the sudden advent of the accumulated torrent at a village some miles below; and the great loss of life in the recent catastrophe at Johnstown, Pa., from the failure of the South Fork Dam, was undoubtedly largely due to the same cause.

The bore in certain estuaries and tidal rivers appears to be due to the same cause.

In a deep stream with a small descent, and consequently small velocity, the effect above described would be complicated with what are sometimes called waves of translation, which cause a rise in the surface of the stream ahead of the arrival of the additional supply.

DISCUSSION.

ELIOT C. CLARKE, M. Am. Soc. C. E.—I understand how the increased value of R with each added volume of water from the point of supply would make the upstream portion of the torrent tend to overtake that which was flowing ahead of it. But, on the other hand, the effect of this would be so to pile up the water at the lower part of the torrent as to increase greatly the other factor I, in the formula. Then the front part would travel the faster, and not until the inclination due to this increased velocity at the front had subsided, would the water in the rear again begin to overtake that ahead of it. It seems as if theory required that alternately the upper and lower portions of the torrent should flow the faster.

Mr. FRANCIS.—I have no doubt it is on an inclined plane in front, but still the water behind would be perpetually catching up to it.

Mr. E. C. CLARKE.—Is it not a fact that there may be sudden great freshets in the upper portions of streams and yet little rise in their lower portions? If so, the rapid advance of the upper portion of the freshet must be retarded in some way, so that the inclination is reduced and the flood comes down gradually.

Mr. FRANCIS.—These floods that we hear of in the mountainous regions of the West sometimes come down when there is no water in the stream to begin with. One described to me occurred where they were building a bridge over a dry channel; the water came down in a torrent, without warning, and swept away the derricks, stagings and part of the work.

J. P. FRIZELL, M. Am. Soc. C. E.—The reasoning adopted by Mr. Francis, in explanation of the movement of sudden floods in the channels of streams, is strictly analogous to that used in elucidating the phenomenon of the "bore" in tidal rivers. The analogy is not broken by treating the facts he refers to as instances of the flow of water in channels, while the bore is regarded as a case of the propagation of waves.

This remarkable phase of tidal movement occurs on but few rivers, notably the Hoogly Branch of the Ganges, on the Seine, and on the Amazon. It shows itself in its full strength on these, only at the time of spring tides. At the mouth of the river, shortly after the time of the tide, a definite wave is seen advancing up the channel. It increases in height as it proceeds till long after the period of high tide is past at the river mouth. It advances, breaking with a loud roar, having low water in front of it and leaving high water behind it, till it exhausts itself in the upper reaches of the estuary. This fact is accounted for thus: The velocity with which an undulation is propagated in water of

limited depth increases as the depth increases, but does not depend upon the amplitude of the oscillation, so long as the latter is small as compared with the depth. We may regard the tide as rising by a succession of indefinitely small instantaneous steps or jumps. Each successive increase of depth would extend up the channel according to the law of wave movement, each moving a little faster than the preceding. The result is that all unite and move up the channel in one grand wave. The velocity of the main wave is not affected by its amplitude, but only by the depth of water in front of it, so that it is always open to accessions from below. This is precisely similar to the reasoning of Mr. Francis except that he admits that the wave front increases in velocity as its amplitude increases, which is undoubtedly correct, as its amplitude is out of all proportion to the depth of water in front of it.

Analogous behavior is observed on great rivers in case of ordinary floods. It is a fact well known to boatmen that the current is much stronger on a rising river than on a falling one, or when the river is at a stand. This is perfectly explicable on the principles alluded to above, viz., that oscillations and changes of depth propagate themselves more rapidly as the depth increases. This action tends to concentrate the fall in front of the flood. If we fix our attention on any point of the stream, we can readily understand how the velocity there, on the same stage of water, can be greater when the front of the flood is at the point than when it has advanced further down the stream. In the latter case, the slope at the point of observation has greatly diminished. The supply has fallen off. The velocity at the front of the flood is maintained in large measure by the volume of water accumulated in the channel, although the velocity at the front of the flood is unchanged, it is constantly diminishing from that point up to the point of observation, without regard to the effect of influent streams.

In the case of a bursting reservoir, the statements of witnesses are, generally, to the effect that the flood advances in the form of a wall of water. This phenomenon is not to be explained on precisely the same principles as the tidal bore, because the movement is wholly one of translation, the initial depth of the stream being generally insignificant, whereas, motion of translation cuts but a small figure in the case of the bore. The action, in this case, appears more analogous to that of a breaking wave. Waves break on a shelving beach. They break because they have reached a depth in which the cyclic movement constituting the wave is no longer possible. The fluid mass, suddenly freed from cyclic motion, moves forward under the simple action of gravity, very much as it would if suddenly released from confinement by the breaking of a dam, the bottom retarded by the friction of the ground, the upper parts tending to outstrip the lower, and so combing over and breaking. The movement rapidly exhausts itself because it takes place on ascending ground. If we could conceive of its occurrence on de-

scending ground, with the infinite mass of the ocean behind it, we could assign no reason why it should not continue indefinitely.

In the case of a finite mass of water, the breaking action would exhaust itself rapidly after the exhaustion of the supply. The depth would diminish from the front of the flood upstream. All parts of less than a certain depth would lag behind. The mass of water would simply strew itself along the channel, and when the breaking action at the front was exhausted, the whole movement would proceed as in the case of an ordinary freshet resulting from natural causes.

MANSFIELD MERRIMAN, M. Am. Soc. C. E.—Speaking the other day with a gentleman who has had extensive experience in the location of railroads in our western territories, he stated that the same phenomenon had been observed by him in cañons. Often when the water was very low in the streams, a vertical wall of water would come down, due to a local rainfall several miles up stream; and this was so common an occurrence that in pitching their tents for the night they took care to place them so that they would not be carried away. His description of the vertical wall of water agrees with what has been stated of the Conemaugh flood, that the upper portion of the crest of the wall of water was of a rolling appearance; as he said, it resembled the heads of buffaloes pitching over.

The complete explanation of this phenomenon is doubtless of a very complex character, being a problem in the theory of the non-uniform flow of water to which the formula $V = C\sqrt{RI}$ only approximately applies. The paper of Mr. Francis contains the only algebraic discussion of the subject that I have seen, and as a general explanation it is very satisfactory.

Mr. FRANCIS.—In the Conemaugh flood the effect of the break of the South Fork reservoir dam was felt most disastrously in the river below. In one of the early reports on the reservoir its storage capacity was estimated at 525 000 000 cubic feet; at the time of the break it was about 8 feet above the level of the wasteway, which added 152 000 000 cubic feet, making the total contents of the reservoir 677 000 000 cubic feet. This all went out out through the breach in about an hour, making the average rate of discharge about 188 000 cubic feet per second. Water was flowing into the reservoir from the water shed at the same time at the rate of 8 000 cubic feet per second; this, of course, was discharged with the storage, making the total average discharge through the breach about 196 000 cubic feet per second.

The breach was formed by the water flowing over the top of the embankment and washing away the earth; this was, of course, done gradually. As the breach enlarged the quantity of water discharged would increase up to the point when the diminished height in the reservoir would so reduce the velocity as to compensate for the increased

size of the breach. At this point the discharge would be a maximum. I have no means of estimating with any precision this maximum discharge or the time at which it occurred, but I think it must have been nearly double the average discharge, certainly not less than 350 000 cubic feet per second, and the time about thirty minutes after the commencement of the breach, or say at about 3.15 P.M., May 31st. The streams below were in very high flood at the same time from other watersheds. The South Fork River from the reservoir to South Fork Railroad station, where it joins the Conemaugh River, is about 2½ miles, and the first appearance there of the flood caused by the break in the reservoir dam was a great wave through the South Fork, which carried away the railroad bridge over the Conemaugh at 3.08 P.M. This was at least twenty minutes after the commencement of the breach, but before the discharge had reached a maximum.

The flood in the Conemaugh River caused the water to be 4 to 5 feet deep in the lower streets of Johnstown before the South Fork wave reached there, which was about 3.56 P.M. or about one hour and eleven minutes after the commencement of the breach in the dam and about forty-one minutes after the discharge had reached its maximum as estimated above. The distance by the river from the dam to Johnstown is about 13 miles and the descent about 380 feet. If the first wave at Johnstown corresponded to the maximum discharge at the broken dam it traveled at the rate of about 19 miles an hour.

Mr. Thomas P. Roberts, Chief Engineer of the Monongahela Navigation Company, has endeavored to ascertain the time when the flood from the South Fork reached Johnstown, and informs me that, "as is to be expected, the statements are conflicting as to time, depending on the location, *i. e.*, distance up or down the stream, with reference to the railroad station at Johnstown, accuracy of the clocks and watches at the moment of stopping," etc. W. S. Jarboe, M. E., member of our local society of engineers, tells me that he saw at least twenty clocks and watches which had been stopped by the flood, and he made special inquiry on this point, and I rely chiefly on his statement.

The time of the flood, *i. e.*, when it had reached a height sufficient to overturn houses in Johnstown, opposite the Pennsylvania Railroad depot (which point is more than a mile below the beginning or upper end of the built-up part of the town traversed by the flood), was just about 3.56 P.M., May 31st.

I think the time occupied by the water in rising to such a height in the case of the majority of the structures was not more than two minutes, if indeed so much. The time at which the flood reached its maximum height at Johnstown would be difficult to learn. I think most of the houses were gone before the full height was reached, as this was the first wide expanse of the water; the first or violent effect passed, it rose more gradually to the maximum height. The railroad

bank and the stone bridge acted as a dam, converting the entire area into a lake, through which, however, swept very powerful currents, creating the "whirlpool" mentioned by so many witnesses.

All the evidence appears to agree in the statement that the flood advanced down the valley with quite a head, as much as 10 feet, I imagine—most of the eye-witnesses assert that no water was visible in the advance of the wave (I now refer to the valley above the town)—but simply a moving mass of tree trunks, logs, etc., "boiling up" violently, and "producing a cloud of spray."

The Conemaugh flood differs from the case contemplated in the paper, being complicated by the great flood from other sources than the breach in the dam, but although no doubt much modified, there does not appear to have been anything observed which conflicts with the principles discussed in the paper.

The problem in a more general form would be to find the velocity V, after a given time if R increases uniformly or according to some other law, but I have been unable to resolve it into this form.

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DISCUSSION
ON
AMERICAN RAILROAD BRIDGES.*

H. D. BUSH, M. Am. Soc. C. E.—1. *Historical.*—Mr. Cooper's valuable paper on American Railroad Bridges should have as complete a discussion as possible. The difficulty of getting facts concerning the historical portion of this subject will increase rapidly as time goes on. I should not be faithful to my native city if I did not call attention to the work done in Springfield, Mass., in the development of the American bridge.

Springfield, on the banks of the Connecticut River, in the direct line of travel from Boston to the West, was one of the first places having a large bridge. This was built in 1805 (Mr. Cooper refers to it as being built by a Mr. Walcott), and consisted of six arches, which must have been of nearly 200 feet span.

The travel was said to be "neither on the bottom or top chord, but ascending and descending with the curve of the arches on each span." This bridge failed in July, 1814, by crippling of the arches.

In 1816 a Burr truss bridge was built on this site by Captain Isaac Damon, of Northampton, Mass., "a man of great capacity for construction and superior workmanship."

* American Railroad Bridges, by Theodore Cooper, Transactions, Vol. XXI, No. 418, July, 1889.

This bridge consists of six spans of about 100 feet each, and three of 180 feet each, total length. There are three trusses, the center one having two arches side by side. This bridge is still in use, and carrying two horse-railway tracks in addition to ordinary travel.

About 1881 a 5-foot sidewalk was added on the outside of the southern truss, this truss being strengthened by the addition of a Towne or plank lattice truss bolted to it. This bridge bids fair to outlast the present century.

William Howe, the inventor of the Howe truss, lived at Warren, Mass., and came to Springfield in 1840, when Amasa Stone and D. L. Harris formed a firm to build bridges under Howe's patent. This was probably the first bridge-building firm in the United States organized practically, as our bridge companies are now, with an office force of contracting agents, draughtsmen, etc. (a shop turning out iron-work), and a corps of experienced erection men. From this parent firm in Springfield there branched out, as bridge builders, Joseph Stone in Eastern New England, Daniel Stone in the Middle States, A. B. Stone and L. B. Boomer in Chicago, Thacher, Burt & McNary in Ohio, and A. L. Maxwell in Knoxville, Tenn.

Of the original firm Amasa Stone went to Cleveland, Ohio, where, as President of the Lake Shore Railroad, he had the Ashtabula Bridge constructed, and D. L. Harris became President of the Connecticut River Railroad. The original firm has been succeeded by Harris & Briggs; Harris & Hawkins; Hawkins, Herthel & Burrall and R. F. Hawkins, who now is building iron bridges near the site of the original shop in Springfield.

Of the early bridges built by this firm, two deserve special notice. One crossed the Connecticut River at Springfield, carrying what is now the Boston and Albany Railroad, and consisted of seven spans of 180 feet each of double intersection Howe trusses. This was the first Howe truss railroad bridge ever built, and was replaced by a more modern Howe truss of single intersection form in 1855. This, in its turn, was replaced in 1873 by the double track riveted bridge, to which Mr. Cooper has referred. The widening of the old piers for a double track bridge presented some difficulties which were met by a new departure in coffer dams, designed by Mr. Burrall, of Hawkins & Burrall (now consulting engineer for Mr. Hawkins), and described by him in Van Nostrand's magazine at that time.

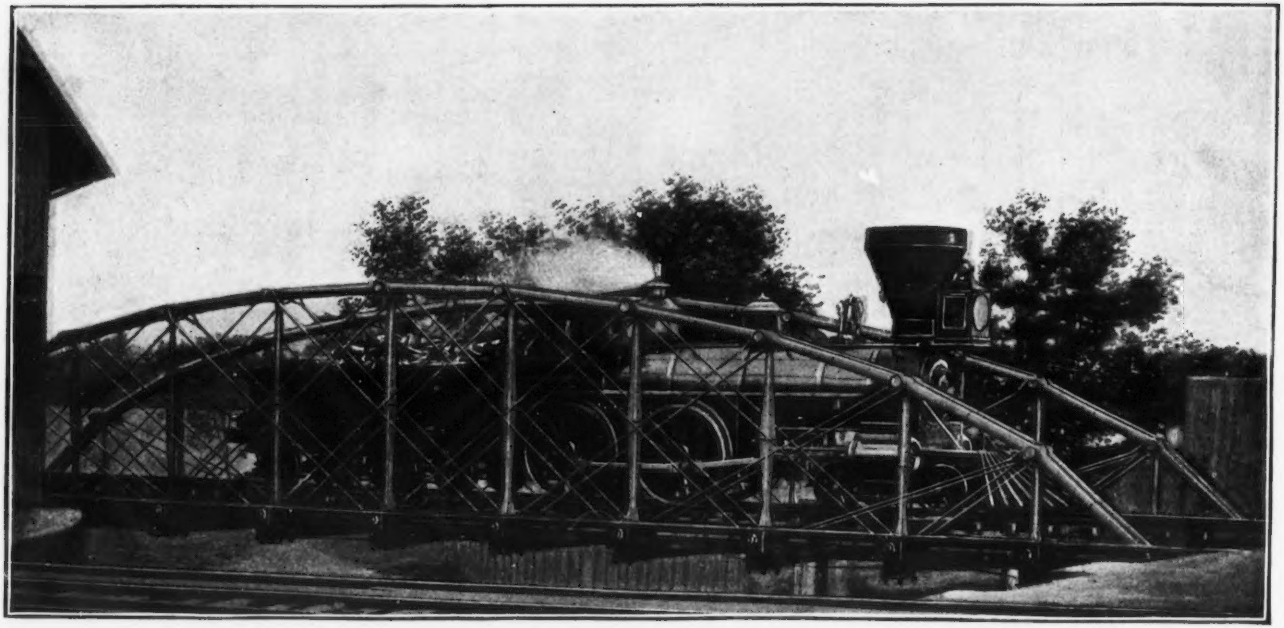
The second bridge was one of two spans of about 260 feet each, at Bellows Falls, Vt. These were Howe trusses, strengthened with arches, and though probably the longest railroad spans of the kind ever built, were in use under the very considerable traffic at this junction point of several railroads until 1882, when they were replaced by two riveted lattice spans of the same length.

The Springfield bridge builders early turned their attention to iron bridges, and, in 1849, constructed an iron Howe truss bridge of double track for the Boston and Providence Railroad. This bridge was of about 50 feet span. The brace and counterbrace of each panel were cast together in one piece, and were of cruciform cross-section. The bottom chord was made of flat iron, with a peculiar form of joint, shown with other details of construction in accompanying drawing. (Plate XCI.)

In 1866 the Herthel Parabolic Truss was patented by George P. Herthel, of Hawkins, Herthell & Burrall. Mr. Herthel was an able engineer, educated at Carlsruhe, and, in this truss, used cast-iron to the best advantage. Though, at this date, wrought-iron bridges were coming into general use, it must be remembered that this material was very expensive in New England, and credit should be given to men who tried to use cast-iron intelligently. Many bridges of the original Herthel truss or its modifications, with straight top chord, made by engineers William H. Burrall and Phelps Johnson, of the Springfield works, were built. The accompanying photograph (Plate XC) shows one of the original bridges still in use after twenty-one years continuous service on a side track of the Connecticut River Railroad over the entrance to Hampden Park in Springfield.

2. *Typical American Bridges.*—The ancient discussion concerning pin-connected and riveted trusses was called American *vs.* English bridges. Mr. Cooper recognizes that there has been a distinctive American practice in the building of riveted bridges, but, I think, does not give the builders of these bridges sufficient credit in the evolution of our modern bridge. He acknowledges the excellence of the early riveted trusses, beginning in 1859, and I think any person who looks into the matter carefully, must acknowledge that, until within the last few years, the average riveted structure has been as good, if not better, than its contemporaneous pin bridge. This is especially evident when we consider what a large percentage of the early pin bridges were constructed of "forms requiring cast-iron joint boxes, now obsolete," and that nearly all of them, so great was the confidence in their theoretical perfection, were made excessively light throughout, to say nothing of their defective details, as for instance, unstiffened end panels and deficient lateral systems attached to hanging floor-beams.

The railroads buying riveted bridges have always insisted on heavy work. In 1880 the Boston and Albany Railroad's specifications prescribed a load of 3 000 pounds per foot with excess loads from heavy engines, and allowing a tensional strain of only 7 500 pounds per square inch of net section. The Bellows Falls riveted spans were figured for a load of 2 750 pounds per foot (with customary engine excess loads), while a year or two later a much more important bridge, the Niagara Cantilever, supposed to be an example of the best practice in pin bridges,



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HERTHEL PARABOLIC TRUSS.

PLATE XC.

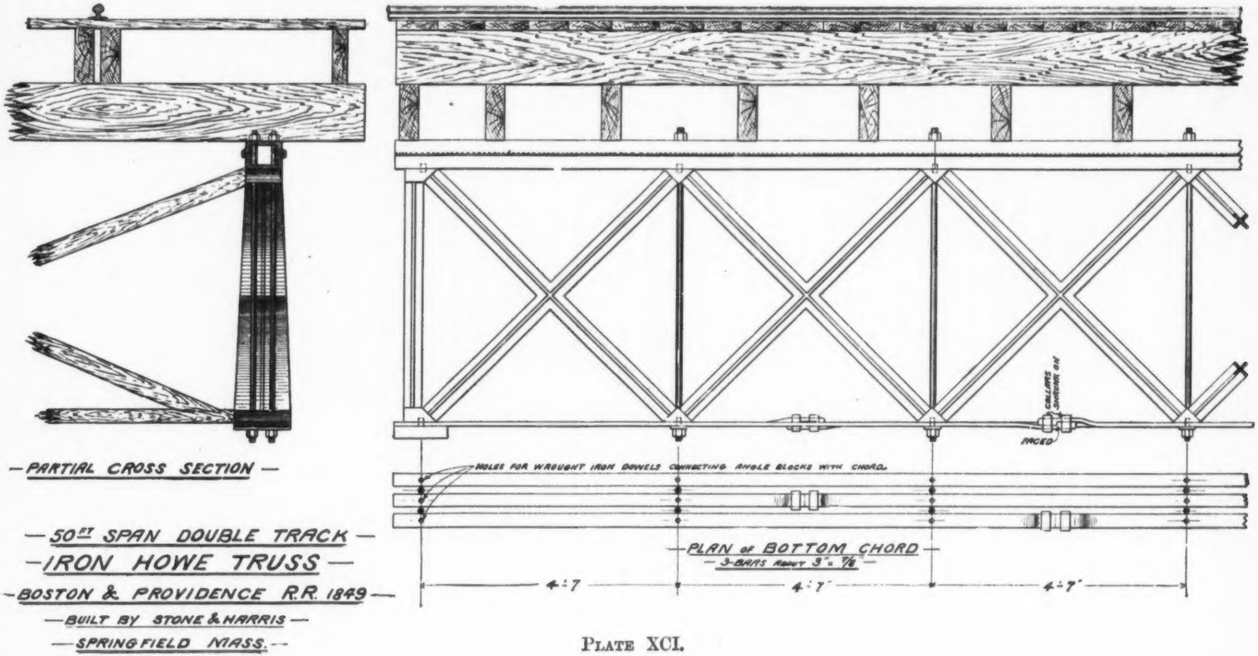

— PARTIAL CROSS SECTION —
— 50 FT SPAN DOUBLE TRACK —
— IRON HOWE TRUSS —
— BOSTON & PROVIDENCE R.R. 1849 —
— BUILT BY STONE & HARRIS —
— SPRINGFIELD MASS. —
COLLARS SHRUNK ON
FACED
HOLES FOR WROUGHT IRON DOWELS CONNECTING ANGLE BLOCKS WITH CHORD.
— PLAN OF BOTTOM CHORD —
— 3-BARS ABOUT 3" x 7/8" —
4' 7
4' 7"
4' 7"

Plate XCI.

was designed for a load of only 2 000 pounds per foot, headed by comparatively light engines.

In spite of its stiffness, which we used to be told was "no indication of strength;" in spite of the evil drift pin, and the fact that "even the fiercest inspector cannot keep every hole and rivet before him," riveted bridges have continued to stand up well under their work, and the loads of abuse heaped upon them. The late very extensive alterations made on the Springfield Bridge of the Boston and Albany Railroad failed to reveal the existence, to any appreciable extent, of any of the evils supposed to be inherent in this form of bridge; and the fact remains that the skilled riveters and heaters employed by the firms building riveted bridges exclusively, invariably do better field riveting than the comparatively unskilled gangs of "climbers" and laborers usually used in doing the very considerable amount of riveting in the modern pin bridge.

However, though the writer's first work was on riveted bridges, and though he now happens to be building fourteen spans of riveted railway bridges, all of over 200 feet each, and in which all compression joints are faced, and work so laid out that separate members can be carefully inspected as to length and accuracy of workmanship, he is willing to acknowledge that in designing for the heavy loads of present specifications, the best type of the modern pin bridge is superior to any riveted bridge in spans of over about 125 feet, or say 100 feet in double-track bridges.

But it must be remembered that the modern pin bridge owes much of its excellence to the adoption of features formerly condemned in the riveted bridge.

Floor-beams are now almost universally riveted to the posts calling for much field riveting in important "joints," and giving rise to a certain amount of ambiguity of strain; stiffened or riveted end lower chords and collision struts are used, and the old-fashioned angle-iron lateral bracing of the riveted bridge is coming into general favor—even Mr. Cooper now specifying that "preference will be given to lateral bracing in the floor system, which is capable of resisting both compression and tension."

The old bugbear of the riveted bridge now applies with double force, for the reasons above given, to the modern pin bridge. This is the uncertainty of hand riveting in erection work, and the writer does not understand why "the engineers acting for the railway companies" have not specified that the bulk, at least, of this riveting should be done by power, instead of by hand, and is confident that American ingenuity would soon produce machines capable of doing this work well and cheaply.

3. *The Manufacture of Bridges.*—Mr. Cooper's description of an American bridge shop might be misleading to one not familiar with our lead-

ing works, and leave the impression that seven or eight different shops were generally used. All work, except forging and lathe and planer work, is generally done under one roof. In at least one shop, that of the Dominion Bridge Company (Limited), specially built by an American engineer for the manufacture of American bridges, all iron work is done under one roof, there being a separate shop for template making only.

In the various departments which Mr. Cooper names there are likely soon to be many changes. In the straightening department, even at this late date, plates are generally subjected to the barbarous process of hammering and "peaning," instead of being straightened both sideways and edgeways by the rolling process in use by the Dominion Bridge Company, and now adopted by one only of the new American shops.

In the rivet shop, punching and reaming in "high" steel work is likely soon to be superseded by drilling from the solid. At least one of our leading engineers is about ready to call for this in his specifications, and some of the manufacturers are in favor of it.

This will at least prevent, in this class of material, the use of questionable processes of straightening angles curved by punching.

In the manufacture of eye-bars a good deal of success has been obtained with rather crude processes and roundabout methods. However, no shop is now ready to make the largest bars now called for, and one of our leading shops has lately been compelled in the long and very heavy span of the Wheeling Bridge to adopt the somewhat doubtful expedient of the use of two lower chords, one above the other.

The probability is that with increasing development and cheaper money such very heavy long-span bridges as are now building and talked of will become more and more frequent, and the probable general use of steel will call for a considerable remodelling of our bridge shops. More capital will have to be employed, and more shop capacity used to keep up the present output, to say nothing of increasing it.

In short, the complete history of the American Bridge cannot now be written.

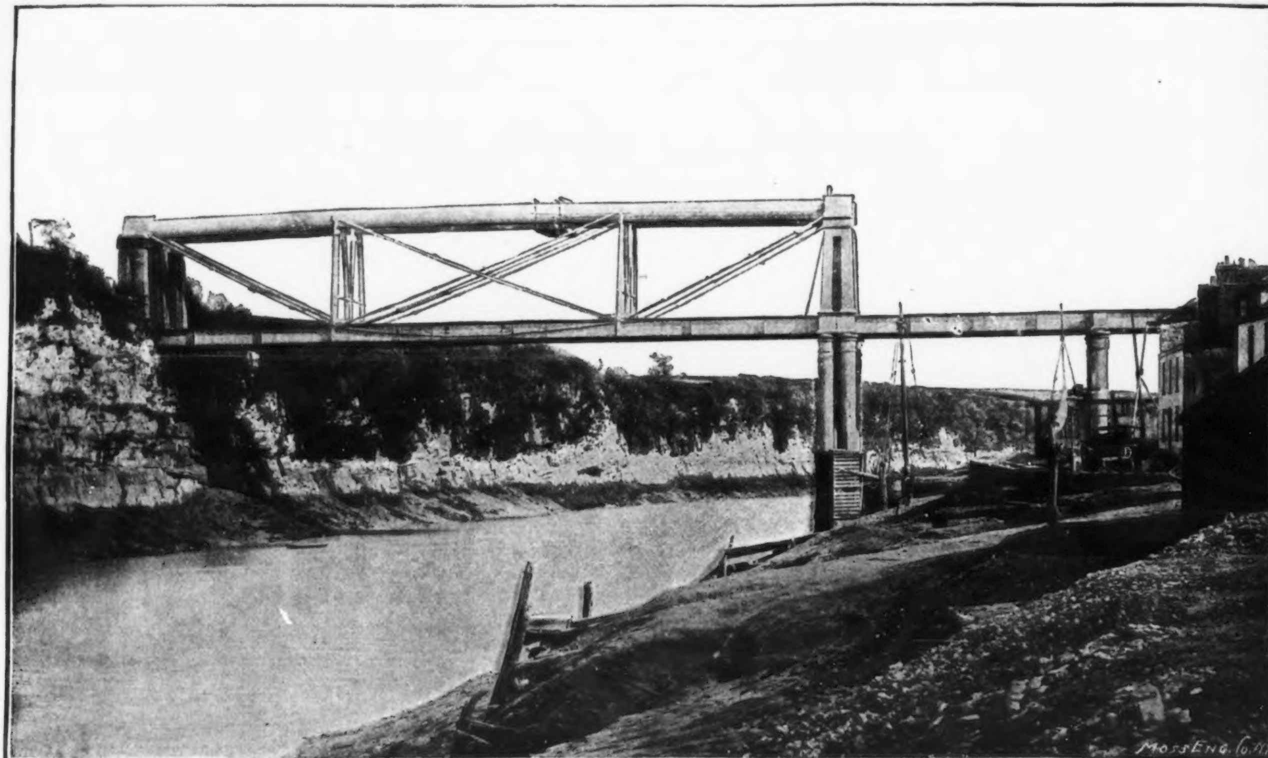
We are now at the opening of one of the most interesting chapters, and there is still great need of the ingenuity and energy of the pioneers to whom Mr. Cooper pays such high tribute.

J. H. Cunningham, M. Am. Soc. C. E.—May I say that as an Englishman, I hope many English engineers will read Mr. Cooper's description of modern American practice in bridge building, and his remarks on "Bridge Failures," for I believe that what he has written may clear away prejudices against pin-connected work and misunderstandings as to what it really is, which at present sometimes prevent its adoption in circumstances in which it might be used with great advantage.

The year 1852 may, I think, be regarded as an epoch in the history of bridge building, both in America and in England. Mr. Cooper tells us that

in America, about that time, the bridges designed by Messrs. Fink and Bollman first came into use, and the earliest Pratt and Whipple trusses were built. About the same time, in England, Mr. Brunel completed his bridge across the Wye, at Chepstow (Plate XCII). This was a striking departure from the tubular system, which, at that time, had recently been introduced with much success. Mr. Brunel reduced the diameter of the tube so that it formed the top boom of a truss which he divided into three panels, each 100 feet in length. The only description of this bridge which I know of is to be found in the life of Brunel, published by Longmans, but its details are very similar to those of the Saltash (Royal Albert), drawings of which are given in Humber's work. The Saltash bridge was opened in 1859. It has two spans of 455 feet, and the track is carried by main parabolic (Pauli) trusses, 56 feet deep, divided into ten panels of 39 feet 4 inches, and two of 28 feet 6 inches. The tension members are flat bars 7 x 1 inch, connected to pins $4\frac{5}{8}$ inches diameter. Thus long panels, flat eye-bars rolled in one piece, and pins were introduced in England more than thirty-five years ago. Brunel died a few months after the completion of this work, and unfortunately the line he struck out in these large bridges was not followed up. Short panel trusses of moderate depth fully riveted became fashionable in England, and continue in vogue, though in recent years there has been a tendency toward longer bays and greater depths.

JOHN STERLING DEANS, M. Am. Soc. C. E.—I have read with much pleasure Mr. Cooper's paper upon "American Railroad Bridges," and particularly that portion in which he so ably presents the merits of "American Designs." As Mr. Cooper there emphasizes, the final erection of the structure must always be kept in view, from the skeleton diagram through all the details of connection. It is this facility of erection, in addition to reducing the ambiguity of strains to a minimum, to which the American pin-connected truss owes its popularity among bridge engineers. As an instance of rapid erection, considering the magnitude of the structure and great size and weight of individual pieces, the erection of the channel span of the Ohio River Bridge, at Cincinnati, by the Phœnix Bridge Company, in 1888, and to which Mr. Cooper calls attention as the longest and heaviest pin-truss span in existence, is believed to be the greatest feat yet accomplished in bridge construction. This whole structure, of over a mile in length of double track and double roadway, containing novel and interesting points, will, no doubt, form the subject of a paper to be presented to the Society in the near future. But it seemed appropriate to add this one item of the channel span erection to the discussion of Mr. Cooper's paper. As is well known, the false work under this span was washed out clean on August 26th, 1888; the Phœnix Bridge Company losing complete false work, traveler completely rigged, and thirteen panels of floor of new span, 650 000 pounds. The company immediately put themselves in



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CHEPSTOW BRIDGE.

Plate XCII.

correspondence with lumber companies throughout the country, and in just five weeks, by the greatest effort, and working continuously by aid of electric light at night, had received and driven 1 200 piles 50 feet long, framed and erected 550 feet of false work 85 feet high, containing 700 000 feet B.M. of lumber; sheathed and braced 1 000 lineal feet of pile protection; framed, erected and rigged a traveler 114 feet high, 55 feet wide, 65 feet long. In the meantime shops had replaced the lost iron of floor, and the first iron was run out September 30th, at 11 A.M., and the span was swinging October 30th. During this time there was a loss of four days owing to heavy rains. This span is, for double track and double highways, 545 feet long, 84 feet deep, 30 feet center to center trusses, 65 feet center to center sidewalk railings, and weighs 3 800 000 pounds. When it is remembered that when this span was swung, it was complete (except roadway brackets) and in working order and carried trains twenty minutes afterwards, all eye-bars, bracing, laterals and struts in position, it must be acknowledged that it reflects great credit, not alone on the engineering firm who designed and built the great span upon the very latest and best single intersection American pin-connected plan, but also upon the American workmen whose skill and energy made such a record possible.

William E. Hoyt, M. Am. Soc. C. E.—One point that Mr. Cooper makes in his paper might, perhaps, be treated a little more fully. It is that well-designed and well-built pin-connected bridges are quite as capable of withstanding sudden shocks, resulting from derailments or collisions, as those of the riveted-lattice type. Since testimony to the contrary has been given by other writers, it might be well for some of us who have had experience in these matters to support Mr. Cooper's argument by evidence in its favor.

I am able to recall a considerable number of serious derailments on bridges that have been under my care, and at least two collisions that happened in through bridges. From a careful consideration of the results of the accidents that have come under my observation, I am convinced that pin-connected bridges have invariably withstood such extraordinary strains fully as well as the riveted-lattice forms. In one instance, particularly, it seems almost certain that the accident would have caused more serious damage to the structure if it had happened on a riveted lattice instead of a pin-connected bridge. In this case, a freight train was thrown entirely through the web of one truss, bending an intermediate post and the suspension bars. The shock knocked one end-post also clear from the shoe, so that the bottom of the broken post stood on the masonry, and yet the bridge in this condition carried trains for some hours afterwards. It was a light structure of its kind, without any special merits of design or construction.

If testimony of this kind is needed to prove the stability of the so-called "American type" of railroad bridges, let us have more examples.

Major O. E. MICHAELIS, M. Am. Soc. C. E.—The acknowledged repute of the author and its own intrinsic merits will undoubtedly give this paper in coming years the weight of accurate history, and I trust Mr. Cooper will pardon me if I call his attention to a matter which he has very modestly omitted to dwell upon—a matter which concerns one of the most interesting periods in American bridge building, and which should certainly have mention in this paper. I allude to the remarkable work done by our bridge engineers during the war; and in this connection the name of one of the most prominent and regretted members of our Society deserves to be recorded. I allude to Colonel W. W. Wright, who was General Sherman's chief bridge engineer in his march from Chattanooga to Atlanta. I especially recall the rebuilding of the very long bridge over the Chattahoochie River. Colonel Wright put that bridge up in eighty-four hours, in the very presence of the enemy. I trust that Mr. Cooper, in the revision of his paper, will say something regarding the work of our deceased member, a man who died in sickness and penury, unknown to the friends who would have tenderly nursed and cared for him, yet whose name deserves to be placed upon every soldier's monument from Maine to California.

THEODORE COOPER, M. Am. Soc. C. E.—Some years ago I, by deputy, sought to collect a great deal of the bridge history of the war, and it was surprising how little I succeeded in getting. If Major Michaelis will exert his influence in that direction, I think he could make a very interesting paper, but in the rush of the war the records of such things pass into tradition. Mr. Stauffer, at my suggestion, some two or three years ago, endeavored by correspondence to secure some records of bridge building performed by our army, and utterly failed. If Major Michaelis can reach them, however, they would be very valuable to have.

J. ELFRETH WATKINS, Assoc. Am. Soc. C. E.—I have been particularly interested in the paper which has just been read to the Society by Mr. Cooper. In the section of Transportation and Engineering in the National Museum which is under my charge we are making an effort to preserve the history of transportation by locomotive and steamboat, and in the models and drawings and originals that we have there we are attempting to make some advance upon what is done at the Patent Museum at South Kensington, the Conservatoire des Arts et Metiers at Paris, or the Naval Museum at Venice, where similar objects are exhibited more as curiosities or relics. As some of you perhaps know, we are attempting to arrange these objects so that we shall be able not only to preserve their history, but to show the progress of thought from time to time. We are especially deficient in matters relating to bridge work; in fact, our collection so far is only a nucleus; for this reason I have been greatly interested in hearing this paper read, and shall take pleasure in preserving the illustrations in our files.

George H. Pegram, M. Am. Soc. C. E.—The concise history and statistics of American bridge development presented in this paper must be of great interest and value to all engineers. The author's long experience and wide and intimate acquaintance with the engineers and engineering of the country have peculiarly fitted him for the preparation of such a paper.

In submitting a few ideas at variance with some of the views expressed, I would not be misconstrued into a want of appreciation of what has been done for us, or as descending from the high plane of the paper to the consideration of a few details of minor importance; but it is such occasions as the discussion of this paper that one must take to bring himself into line with his fellow workers.

Live Load.—In my discussion of a paper by Mr. Joseph M. Wilson, M. Am. Soc. C. E., on "Specifications for Strength of Iron Bridges," Transactions of the Society for June, 1886, I showed that a uniform load in combination with a single concentrated load would give all of the results obtained by the use of the engine loading with substantial exactness.

Examples were given of spans from 8 to 400 feet girders; and single and double intersection trusses, with a variety of panel lengths sufficient, it was thought, to place the question of the entire equivalence of the two loadings beyond dispute.

The severity of the test was increased by the requirement of Mr. Wilson's specifications that the maximum strains from three different classes of engines should be taken.

This requirement was made by Mr. Wilson to cover the greater concentration of weights on the wheels of some tank and passenger engines. If he had gone a step further and required different positions of these engines with reference to each other and to the train he would have represented still more accurately the loading to which his bridges were subjected. Such a condition would, however, be proscribed on account of the complexity of the specifications and large amount of labor required.

It is, therefore, claimed that the proposed loading through practically representing an average of all of the combinations of loadings gives more accurate results than the engine loading specified, and since through its use the strains can be determined in about one quarter of the time ordinarily taken when only one class of engine is specified, its general use would seem desirable. In still other respects it is better in insuring the same results by all calculators, being independent of prepared tables and diagrams, and allowing a choice of finding the strains by either the analytical or graphical method with equal convenience.

From the table (No. 1) given by Mr. Cooper, showing the increase in the live load for the past seventeen years, we see that if we allow six

months for the time from the commencement of calculations to the completion of a bridge, the loading will have increased in the time 3 per cent. If we further consider that the bridge will last twenty years, in which time the load will have increased 120 per cent., and should last a hundred years except for this growth of the load, there would seem to be little excuse for the refinement to which some calculations are taken, especially when based upon one of many reasonable assumptions with regard to the load.

I would suggest now that a still further advance be made by making the relations between the uniform and concentrated loads such that the effects of impact shall be included.

Impact is known to vary with the length of train motion through which a member receives its maximum stress. It is a custom with some engineers to provide for it by a sliding scale of percentages to be added to the stresses produced by the load considered as static. With some others it is the custom, instead, to vary the unit stresses. Both of these methods involve a complication and inexactness which can, it seems, be avoided in the manner proposed.

As actual cases are most satisfactory in such a discussion, I shall illustrate the point by the Erie loading of 1879. This loading is so much out of date that the results can be of little use, except for illustration, but it will be a more satisfactory test because the difference between the average weight of the engines and that of the train is much greater than in subsequent loadings, making it rather more difficult to approximate with a uniform load, and also because Mr. Cooper has given some tables for this loading with which it will be interesting to compare.

I would propose in lieu of the Erie loading, and as containing provision for impact, a uniform load of 2 400 pounds per foot, combined with a concentrated load of 40 000 pounds, the concentrated load being placed at that point which will give the maximum stress in the member under consideration. A concentrated load passing over the length of the span is equivalent to a uniformly distributed load of double the amount, and hence the flanges of girders and the chords of trusses will be figured for a total uniformly distributed load of $2\,400\,S + 80\,000$, S being the span; and the web stresses of trusses by adding 40 000 pounds to the common panel load at the end of the train and backing it off the span in the usual manner.

In this latter it is assumed that the uniform load will extend over the panel in front of the concentrated load, and the half panel load at the advance panel point shall be neglected, a condition in favor of simplicity and involving no error when it is an understood feature of the calculations.

The following table (No. 1) gives a comparison of the proposed loading with the approximate equivalent loads given by Mr. Cooper for the Erie loading.

TABLE No. 1.—Erie Loading.

Span in Feet.	Equivalent for moments, Cooper.	$2\,400 + \frac{80\,000}{S}$	Percentage for Impact.	For Double Shears. Cooper.	$2\,400 + \frac{40\,000}{S}$	Percentage for Impact.	For Single Shears at end, Cooper.	$2\,400 + \frac{80\,000}{S}$	Percentage for Impact.	For End Web Shears, Engine.	Proposed Loading.	Percentage for Impact.
10	5.290	10.400	96	4.620	6.400	40	7.200	10.400	45	5.940	9.576	61
15	5.280	7.730	55	4.210	5.100	17	6.240	7.730	24	5.280	7 176	55
20	4.640	6.400	38	3.830	4.400	15	5.400	6.400	19	4.817	5.976	24
30	4.200	5.100	20	3.400	3.730	10	4 740	5.100	8	3.866	4 776	24
40	3.850	4.400	15	3.175	3.400	6	4.300	4.400	2	3 554	4.176	17
60	3.400	3.730	10	2.970	3.070	3	3.830	3.730	....	3.146	3.576	10
80	3.260	3.400	5	2.870	2 900	....	3 620	3.400	....	3.100	3.300	6
100	3.230	3.200	....	2.750	2.800	....	3.450	3.200	....	3.200	3.200	0
150	3.070	2.933	....	2.450	2.670	....	3.180	2 933	....	3.048	2 936	
200	2.910	2.800	....	2.370	2 600	....	3.000	2.800	....	2.900	2 900	
300	2.710	2.667	....	......	......	....	......	......	....	2.690	2.666	
400	2.610	2.600	....	......	......	....	......	......	....	2,643	2.600	
500	2.540	2.560	....	......	......	....	......	......	....	2.547	2.560	

Let us consider this table. In the columns for moments the high percentages for impact in spans under 30 feet will provide for the greater concentrations and speeds of passenger and tank engines. They agree well with current practice. The percentages for double shears conform exactly with those for moments; thus for a 10 feet span the percentage should be for a train motion of 20 feet, which it is. Double shears over 100 feet would be of no practical use, except for loadings on piers, where the engines should be assumed to be preceded as well as followed by cars, in which case the proposed loading would be exact.

Mr. Cooper has designated as "single shears" those at the extreme ends of the spans, where one heavy wheel is just moving on to the support. The percentages for these columns should be the same as those for moments, whereas we see that they are much less.

The only use that would ever be made of these values would be in the riveted attachments at the ends of spans generally under 40 feet, as stringers to floor beams. The end shears that would be used to determine the web would be taken, in girder spans, at a distance from the end equal to the depth of the girder, and in truss spans at the first panel point. These are given in the last columns, and are seen to be substantially correct. I think they may also be taken as representing more correctly the shears at the end attachment than those given in the preceding columns, because the latter depend upon an ideal theoretical condition that has no parallel in practice. Taking, for instance, the attachment of a stringer to the floor beam, and assuming that a heavy wheel is over the center of a cross-tie placed close to the floor beam, the distance from the end of the stringer to the wheel becomes one foot, modifying in a considerable degree the percentages given, and if we further take into account the distributing effect of the rail, which Mr. Cooper's specifications place at three ties, we are reduced practically to the conditions in the last column.

The heavy passenger engines referred to, say with 30 000 pounds on drivers 8 feet apart, while producing greater moments than the consolidation engines in small spans, will produce less shears.

For trusses, comparison between the actual stresses in the different members and the amounts, as well as percentages, of difference are so much more satisfactory than those between approximate equivalent loads that, at the risk of being prolix, they are given in Tables Nos. 2, 3 and 4.

The values are for one truss, being those due to half of the loads previously given. The letters *a*, *b*, *c*, etc., refer to the panel points at which the stresses occur, calling the first panel point from the pier *a*, the next *b*, etc.

In spans over 200 feet it is thought necessary to give only the shears in the end panel, the middle panel and the counters.

The values are for *live load only*, and the counters are given to the panel at which the dead load shear overcomes the live.

TABLE No. 2.

CHORD STRESSES FOR ERIE LOADING—SINGLE INTERSECTION.

Panel.	100 feet span, 25 feet deep. 5 panels of 20 feet.			150 feet span, 25 feet deep, 6 panels of 25 feet.			200 feet span, 33⅓ feet deep. 8 panels of 25 feet.		
	Engine Load	Proposed Load.	Pr. ct. Diff.	Engine Load.	Proposed Load.	Pr. ct. Diff.	Engine Load.	Proposed Load.	Pr. ct. Diff.
a	51.160	51.200	0	95.220	92.100	−3	95.700	91.900	−4
b	72.520	76.800	6	147.500	147.300	0	155.400	157.500	+1
c	......	......	....	156.700	165.700	+6	196.500	196.300	0
d	......	......	....	......	......	....	202.090	209.900	+4

TABLE No. 3.

WEB SHEARS FOR ERIE LOADING.—SINGLE INTERSECTION.—ALL 25 FEET PANELS EXCEPT IN 100 FEET SPAN.

	100 feet Span.			150 feet Span.			200 feet Span.		
	Engine.	Proposed.	Pr. ct. Diff.	Engine.	Proposed.	Pr. ct. Diff.	Engine.	Proposed.	Pr. ct. Diff.
a	63.950	64.000	0	95.200	91.900	− 4	126.900	122.500	− 3⅓
b	35.430	40.800	14	62.500	62.600	0	95.900	93.700	− 2½
c	17.440*	22.400	30	36.700	38.900	3	68.400	68.800	0
d	......	......	..	15.300*	20.500	30	46.600	47.500	+ 4
e	......	......	..	......	......	....	25.400*	30.000	+ 19
	300 feet Span.			400 feet Span.			500 feet Span.		
a	185 300	183.400	− 1	247.800	243.700	0	302.600	304.000	+ 1
Mid.	61.800	62.500	+ 1	77.300	77.500	0	92.400	92.500	0
	44.000*	45.800	+ 4	60.120*	61.800	2	60.400*	62.000	3
	28.200	31.700	+ 11	45.000	47.000	5	46.800	49.000	5

* First counter.

TABLE No. 4.

Web Shears for Erie Loading.—Double Intersection.

	200 feet Span. 8 Panels of 25 feet.			200 feet Span. 12 Panels of $16\frac{2}{3}$ feet.			500 feet Span. 20 Panels of 25 feet.		
	Engine.	Proposed.	Pr. ct. Diff.	Engine.	Proposed.	Pr. ct. Diff.	Engine.	Proposed.	Pr. ct. Diff.
b	63.300	61.900	— 2	61.200	67.500	10	152.500	153.800	
c	45.500	44.400	— 2	50 300	55.800	11	137.400	137.700	
d	29.400	34.400	+ 17	42.500	47.500	12	127.200	124.700	
e	19.400	24.300*	+ 25	32.300	37.500	16	110.600	110.200	
f	13.100	14.400	+ 10	26.900	30.800	13	99.000	98.700	
g	......	......	....	19.600	22.500*	15	86.500	85.800	
h	......	......	....	14.800	17.500	19	76.500	75.800	
i	......	......	....	......	......	..	65.200	64.300	
j	......	......	...	.....	......	..	56.700	55.700	
k	......	......	....	......	......	..	46.700*	45.700	
l	.....	......	....	......	......	..	39.600	38.700	

We see that the percentages of excess available for impact in ties and counters decrease with the length of span, but are about right up to 200 feet, and, therefore, to make the loading cover all cases, we should add the specification that 10 per cent. shall be added to live load stresses in ties and counters of spans over 200 feet.

Table No. 4, for double intersection trusses illustrates a defective feature of the engine loading to which attention was called in the discussion of Mr. Wilson's paper alluded to, and shows how the proposed form of load, by covering the maxima of the various cases, meets the point. The centers of gravity of two consolidation engines in the positions specified are about 50 feet apart, and hence the maximum concentrations of weight will occur on the same system of bracing with panels of 25 feet, while with $16\frac{2}{3}$ feet panels they will occur on different systems and give less results than would be obtained with other positions or combinations of engines, a particularly unfortunate state of affairs when we consider that the latter panel is about a half a car length, throwing the maximum concentrations upon one system of bracing while we are calculating for a uniform weight of train, which implies an equal division between the systems, and, therefore, adding to the error. The proposed loading gives the maximum effects regardless of the panel length.

I have not gone into the question of the 101-ton consolidation engine loading, further than to make a comparison with Mr. Cooper's uniform loads for maximum moment, from which it would seem that the proper equivalent, impact included, would be 3 000 pounds uniform plus 50 000 pounds concentrated. As a substitute for the Lehigh loading I would suggest 4 000 pounds per foot uniform plus 70 000 pounds concentrated.

In the following table (No. 5) the equivalent values for the Lehigh engine loading for spans over 40 feet are the averages of those giving the

maximum middle moments and end panel shears. Although the agreement seems quite exact, in the calculation of trusses 3.900 + 70.000 will be found more nearly the equivalent load. The greatest disparity will be found in the chord stresses, attributable to the fact that while we take positions of the engines which we know give maximum web shears, they are positions which we know do not give the maximum chord stresses. For these the engines should obviously be preceded as well as followed by cars.

TABLE No. 5.

EQUIVALENT UNIFORM LOADS FOR MOMENTS.

Span.	101 ton consols. + 3.000 lbs.	$\frac{100.000}{S}$ + 3.000	Per cent. for impact	Lehigh Engines + 4.000.	$\frac{140.000}{S}$ + 4.000.	Per cent. for impact
5	12.000	23.000	92	16.000	32.000	100
10	6.750	13.000	90	9.610	18.000	87
15	6.670	9.666	45	9.600	13.300	39
20	6.190	8.000	29	8.560	11.000	29
30	5.470	6.333	16	7.290	8.700	19
40	4.760	5.500	16	6.690	7.500	12
60	4.110	4.666	14	5.630	6.300	12
80	3.950	4.250	8	5.270	5.750	9
100	3.910	4.000	3	5.070	5.400	6
150	3.740	3.667	−2	4.830	4.930	2
200	3.590	3.500	−2	4.680	4.700	
300	3.390	3.333	+2	4.410	4.470	
400	3.180	3.250	+2	4.300	4.350	
500	3.150	3.200	+2	4.230	4.280	

The high percentages in spans under 15 feet, as before remarked, will serve to cover the greater concentrations and speeds of passenger engines.

From a study of the three loads given it would seem to be a good rule to make the uniform load equal to the weight of the train, and make the concentrated load seventeen times that amount.

The actual wheel loads must, of course, be the basis of any adopted form of loading, and the idea of specifying the simpler equivalent is to avoid the enormous waste of high-priced labor involved in specifying the engine loading, where every engineer competing for the bridge must make elaborate calculations, which had better be made once for all by the party making the specifications, particularly where, as it has been shown, better results may be obtained.

Camber.—In the matter of camber it would seem better to provide for the deflection due to the web members by changes in the lengths of these members, rather than in the chords, as proposed in this paper.

In fact one cannot be made to cover the other because they do not act together, the deflection due to the web members increasing with the depth of the truss, while that due to the chords decreases.

The deflection of a truss being due to the change of lengths of all of its component parts by stress, why not anticipate this deflection by alter-

ing the lengths of all of the members, so that when sustaining its maximum load it shall be of the theoretical figure.

This is really the object of camber, and it would seem the scientific way to provide for it.

Thus, assuming 8 000 pounds per square inch as the average stress in the top chords and end posts, 6 000 pounds that in the intermediate posts, and 10 000 pounds that in the ties and bottom chords, and a modulus of elasticity of 24 000 000 pounds, the alteration would be as follows: Lengthen top chord panels and end posts $\frac{8\,000}{24\,000\,000} = \frac{1}{3000}$ length, and intermediate posts $\frac{1}{4000}$ + ($\frac{1}{50}$ inch play in pin holes). Shorten bottom-chord panels and diagonal ties $\frac{1}{2400}$ length + $\frac{1}{50}$ inch. This presumes that the web members shall be under their maximum stresses at the same time as the chords, which is not strictly true.

Let us consider the members in detail. The counter ties being adjustable are independent of alterations for camber. The main ties at the end are under their maximum stress at the same time as the chords; the main tie at the middle of a 200 feet span, for example, is under but 75 per cent of its maximum at that time. The average stress in the ties is, therefore, $87\frac{1}{2}$ per cent., making an error of $12\frac{1}{2}$ per cent. in the above assumption, which would be exactly compensated in the case considered by disregarding the play in the pin-holes of all the ties. The intermediate posts would be under a still smaller proportion of their maximum stresses, and the correction should be more properly $\frac{1}{4800}$ than $\frac{1}{4000}$.

The camber from the chords would be dependent upon the relative lengths of the top and bottom chords, and would obviously not be affected if we added $\frac{1}{50}$ inch to both chords in a common panel of say 25 feet. This would be equivalent to disregarding the play in the pin-holes of the bottom chord, and making the elongation of the top chord $\frac{1}{2400}$ in place of $\frac{1}{3000}$. But in doing this we react upon the web members by increasing the horizontal projection of those in one panel $\frac{1}{50}$ inch. This will be fairly compensated by neglecting the play of the pinholes in the posts.

We come, then, to the following simple rule which would seem to be as near theoretical exactness as can well be made, viz.: Camber shall be provided by changing the members of the theoretical figure of the truss $\frac{1}{2400}$th of their respective lengths except intermediate posts, in which the change shall be one-half this amount or $\frac{1}{4800}$th; compression members to be lengthened and tension members shortened.

The correction under this rule is simply $\frac{1}{2}$-inch per 100 feet for all members except intermediate posts in which it is $\frac{1}{4}$-inch. The fraction is such a small one that we can neglect the inches in the expressed lengths of the members, and the corrections can be written down from inspection; being a constant for each class of members in a Pratt truss,

the method is thought to be as easy of application as that given in the paper, besides being more correct and independent of the form of the truss and taking into account the play in the pin-holes.

I have taken the modulus of elasticity—24 000 000—as given by Mr. Cooper. This is too small, but provides an excessive camber of about 20 per cent., which it is desirable to have in view of the constantly increasing rolling load, and hence its use.

The members have been assumed to be iron, and when steel is used at higher stresses the elevations under the rule should be proportionately increased; but in the ordinary use of steel eye-bars at 12 000 pounds per square inch, with all other parts iron, it is more convenient, and I am inclined to think sufficiently exact, on account of the slightly higher modulus of steel and the excess which the rule gives, to apply the rule without alteration.

Palmer C. Ricketts, M. Am. Soc. C. E.—Whilst the credit for the development of the method, now generally in use in this country, of calculating the stresses in railroad bridges by wheel concentrations, is doubtless due to Messrs. Cooper and Escobar, the effect of engine wheel loads on spans had been considered abroad by Winkler, Von Ott and others some years before the date (1880) mentioned by the author as the time at which he first used it.

Mr. Cooper is somewhat in error in saying that this method has never been published before, since the principles and conditions as now used were given in an article by Mr. C. E. Moore, in the *American Engineer* of December 19th, 1884, and they have also been ably presented and extended by William H. Burr, M. Am. Soc. C. E., in his "Stresses in Bridge and Roof Trusses" (1886), in which, after a general treatment, he deduces stresses for special engine loads and spans. In both these cases credit is given to Mr. Cooper.

It is well, also, in this connection, to forget neither the valuable paper by Professor G. F. Swain, Assoc. Am. Soc. C. E., published in our Transactions of July, 1887, nor the Treatise on Bridges (1888) of Professor Merriman, M. Am. Soc. C. E.

Henry B. Seaman, M. Am. Soc. C. E.—I would offer a correction to the statement that it is the general American practice to brace our trusses for a definite amount of lateral force per lineal foot of structure. This rule is empirical and is not consistent with the American methods of scientific design. The specification quoted provides for a moving train, ten feet high, with a wind pressure of 30 pounds per square foot, but it makes no provision for the height of the train above the track, which is an important factor in the calculation of the moment of instability in trestle towers. In providing for a uniform pressure of 150 pounds per lineal foot of each chord, irrespective of the length, depth, or details of the trusses, it makes no allowance for the floor system at

the loaded chord, and in the case of check bridges where the floor system and the upper chord act together, the difference between this and the lower chord, composed of eyebars, is very marked. The specification was apparently framed with a view of lessening labor and to facilitate the manufacture of so many tons of iron in a certain limited time. As such, it has naturally found favor with contractors, and may generally be found in their specifications, though to this there is an exception in the specification cited by Mr. E. Claxton Fidler, which shows unusual thoroughness in its preparation and from which engineers have wisely made liberal abstracts. Of several railroad specifications in my possession, including those of the Pennsylvania Railroad and the Baltimore and Ohio Railroad, there are none which adopt this rule, but in each case vary the amount of wind pressure according to the surface exposed.

The objection to the use of engine distribution does not arise merely from the increased labor of the calculation of the strains. The chief objection is the over-confidence it gives in the validity of our results. There is no certainty that the concentrated weights assumed are the same as those to which the bridge is subjected by a moving train, and with the constant increase in the weights and the changes in their distribution, the expedient of engine distribution must be only a temporary one, while the tendency of railroad management is to increase the weights of the engines; there is an equal and less restricted tendency to increase the weights of the trains which follow; and as long as the co-efficient of sliding friction is greater than that of rolling friction, we have no assurance that economy of transportation in reducing the lengths of trains and the increased facilities of our manufacturers requiring the transportation of heavy weights will not ultimately give us trains of equal weights with the engines. It is just announced that a special car has been constructed by the Lehigh Valley Railroad for the transportation of heavy machinery, while two years ago a similar car was constructed by the Pennsylvania Railroad for the transportation of cables. The only limit to be placed upon these loads and their concentration is the strength of the bridges and the solidity of the track. Is it then wise to devote our attention to the detailed effects of engine distribution, ridiculing our factor of safety, while we ignore conditions which already exist and of which we have no knowledge?

C. F. Stowell, M. Am. Soc. C. E.—The value of the historical portion of Mr. Cooper's paper can hardly be over-estimated. He has collected and recorded a mass of facts relating to the early history of American bridge building, which was in danger of becoming permanently lost.

It seems to me that it would have been well to include in his list of early iron bridges the one over the Mohawk River at Utica, on the Rome, Watertown and Ogdensburg Railroad, as being probably the

oldest iron truss bridge now in use for railroad purposes in this country. This bridge was built by the late Squire Whipple in 1854, and is understood to be the second railroad bridge built by him on his well known trapezoidal plan, the first being the one mentioned by Mr. Cooper, on the Rensselaer and Saratoga Railroad, near Troy, and which was removed in 1883. The Utica bridge is 125 feet long, and has cast-iron top chords and posts, rods with screw connections for tension members, and welded links for lower chords. Some time after being erected the span was carried away by a flood, but the pieces were picked up and replaced, and the bridge is doing duty to-day in substantially the same condition as when first built, except that a new and stronger floor system has been put into it.

As regards the date when the modern type of bridge specification first appeared, I think Mr. Cooper is somewhat in error. He claims priority in a number of points for the old Erie specifications drawn up by him in 1878, but I find that the "Specifications for Wrought-iron Bridges" of the Lake Shore and Michigan Southern Railway, which were published at least as early as July, 1877, cover substantially the same points. These specifications covered the designing, proportioning and detail of construction with that completeness necessary to give the railroad company full advantage of the competitive system, and at the same time insure that the resulting structure would not be below the standard. They specified definitely the working strains to be allowed on different parts of the structure, according to the service to be performed, and while stating clearly the exact loads and working stresses to be used and degree of excellence of workmanship to be adhered to, they still left the contractor at perfect liberty to adopt such style, depth and panel length of truss as might seem to him most advantageous. In view of these things, therefore, I think Mr. Cooper errs in claiming for the Erie specifications of 1878 priority in the points just mentioned, and which are now universally incorporated in all the American bridge specifications of any degree of merit. The Lake Shore specifications were drawn up by the late Charles Hilton, under the direction of Mr. L. H. Clarke, then Chief Engineer of that road.

The remainder of Mr. Cooper's paper, after the historical portion, is largely occupied with a description of the processes of computation, manufacture, erection, etc., in common use in American bridge shops, and while in the main accurate and reliable, still this portion contains a number of statements laudatory of the pin-connected type of bridge, and derogatory to its riveted congener, to which, I think, exception may reasonably be taken.

The point usually insisted upon most strenuously by the advocates of the pin-connected system is the asserted ambiguity as regards the stresses in a riveted bridge compared with a pin-connected one, but as Mr. Cooper does not urge this objection I have nothing to say about it

here, except that it is usually much exaggerated. It is not true, as has been asserted by a prominent advocate of the pin-connected type, that it is utterly impossible to make any estimate of the strains in a riveted lattice truss, nor is it true that the stresses in a pin-connected truss can be computed with absolute exactness in every member. There is more or less ambiguity in the stresses in every bridge, and the disparity between riveted and pin-connected ones in this respect is less than some would have us believe.

As an objection to the use of riveted construction for long spans, Mr. Cooper says, "for the larger spans the necessity of performing so much riveting of important connections at the bridge site, where that care and accuracy obtainable at the shops cannot be depended upon, does not render them acceptable to many engineers." I know of no more vital or important connections in a bridge than those of stringers to floor beams and of floor beams to the trusses. These connections must resist the first shock of a locomotive as it jumps upon a bridge, and they get very much more wear and tear and rough usage than any other connections in the bridge. But rivets are always depended upon in first-class, modern, pin-connected bridges for these connections, and not only this but the stringer connections always and the floor beam connections frequently are necessarily made by means of hand-driven rivets at the bridge site. As long as the primary dependence of a train on a bridge always has been and always will be upon hand-riveting done at the bridge site, it seems to me inconsistent to object to "so much riveting of important connections at the bridge site."

The facts about the erection of pin-connected bridges given by Mr. Cooper are certainly remarkable, especially if we are to understand that the spans mentioned were riveted up complete ready for track-laying in the periods stated. But if he means by "erection" the putting together of a truss sufficiently to make it self-supporting, and then doing the necessary riveting at leisure afterwards, the feats mentioned lose somewhat of their brilliancy. It is undoubtedly true that to erect a riveted bridge complete takes longer than for a pin-connected span of the same size, and this is due to the extra time required for riveting. But simply to render a truss self-supporting so that the false work may be removed if necessary, is quite another matter. If this be called "erecting," I am of the opinion that the records of riveted bridges where speed is essential would compare favorably with those of the other type. It is not necessary that all or any field rivets be driven for this purpose. Only a few bolts are needed. It is not even necessary that all or nearly all the members of a truss should be in place. I know of one case where the false work was carried from under a riveted span of considerable size before any of the top chord was in place, but the truss stood without a top chord. A pin-connected truss must inevitably have fallen. Moreover, it should be considered that in the

erection of a riveted lattice bridge no false work is ever required above the lower chord, so that all the time and expense of carrying false work up to the full height of the trusses, as is essential in pin-connected spans, is saved in using riveted work. In cases where it is not desirable to obstruct a waterway at all by false work a riveted bridge may be, and frequently is, built entirely on shore and shoved across when completed. Thus the York street bridge in Buffalo, a 182 feet span over the Erie canal, was built in the street to avoid interfering with navigation by false work, and pushed across when fully erected, the outer end being supported by a canal boat during the transit. A riveted bridge requires no special design or extra members whatever to enable it to be shoved about in this way. After all, though it is true that speed in erection is sometimes of the utmost importance, nevertheless such cases in ordinary practice are comparatively rare. In a great majority of cases any reasonable length of time which may be desired can be taken.

In looking back over the field of American bridge building during the past few years I am fully persuaded that the use of riveted work is gradually becoming more extensive. This is shown (*a*) by the general extension of the limiting span for which riveted trusses are permitted on roads which do not employ them for all spans, (*b*) by the greater dependence on riveted connections in various parts of pin-connected bridges, and (*c*) by the adoption of the riveted type for long span bridges by roads which formerly preferred the pin-connected type. The Erie specifications of 1878 required riveted girders to be used for spans of from 40 to 75 feet, and prohibited them for longer spans. The present Erie specifications require their use up to 80 feet, and for longer spans they are optional. Mr. Cooper's own specifications, dated 1888, require them up to 100 feet, though in 1884 his general specifications put the limit at 90 feet. In 1878 it was considered good practice to depend upon abutting joints in top chord sections with just enough rivets to hold the pieces in place; but now it is stipulated in all good specifications that "all joints in riveted work, whether in tension or compression members, must be fully spliced, as no reliance must be placed on abutting joints." The old Erie specifications required for floor beam hangers only that they should be so placed as to be readily examined, that check nuts shou d be used for screw connections and that bent loops should fit closely around the pins. The use of plate hangers for floor beams, where the dependence is necessarily on rivets, is now almost universal unless special circumstances require some other form. The modern Erie specifications read, "Floor beam hangers [*i. e.*, bent loops or screw connections] shall be avoided if possible," and Mr. Cooper's own specifications of 1888 say, "Preference will be given to hangers without screw connections."

When the Erie specifications of 1878 were published the bottom lateral bracing of through pin-connected bridges was always com-

posed of round or square bars with pin or screw connections, in the attachments of which, by the way, the specified prohibition against using rivets in direct tension was almost always conveniently disregarded. But in Mr. Cooper's 1888 specifications, paragraph 8 says: "Preference will be given for a stiff angle-iron lateral system between the chords on the level of the floor." And again in paragraph 24: "Preference will be given to lateral bracing in the floor system, which is capable of resisting both tension and compression." In first class work the practice of stiffening the lower chord in the end two panels of pin-connected bridges is now becoming common. The Lake Shore and Michigan Southern Railway, for instance, makes it a specified requirement in all cases where the first panel point is a suspender only. These things go to show that not only is the limiting span for which riveted girders proper may be used being gradually extended, but that the use of riveted construction and the dependence in pin-connected bridges upon riveted connections, where other forms were formerly considered better, is becoming more and more common. In all the points mentioned where riveted work is making inroads into pin-connected construction —in the use of fully spliced top chord sections, in the dependence of floor beams upon riveted connections instead of bolts and nuts, in the use of stiff lower chords and of rigid lateral bracing—in all these things the tendency is precisely towards the practice which is now and has been for years universal among the builders of riveted bridges. But besides this the use of riveted work is becoming more general for entire spans even of considerable size on roads which until comparatively recently used only pin-connected bridges. As an instance of this I need only mention the new bridge over the St. Lawrence River now being built by the Canada Atlantic Railway. This bridge will comprise one span of 163 feet, two of 190 feet, ten of 217 feet, four of 223 feet and one draw span of 355 feet in the clear. All the iron bridges heretofore built on this road are pin-connected, but this bridge, I am informed, is to be entirely of the riveted lattice type, the management of the road having wisely decided upon that style of construction as presenting a greater measure of safety in the event of a train accident on the bridge.

Even admitting all that the advocates of the pin-connected type have claimed in favor of their structures and against the riveted type to be true, still it seems to me that the latter would be preferable for railroad purposes, for the single reason indicated as governing the choice in the case of the Canada Atlantic Bridge.

The paramount consideration in the building of any bridge must always be safety. No matter to what degree of refinement the computation of stresses may be carried under a system of wheels placed just so many inches apart, and just so many pounds on each wheel; no matter how careful the inspection of material and workmanship or how much pulling of test pieces may be done; no matter how many engineers may

have pronounced the finished structure as possessing the utmost degree of excellence, still I maintain that if some other type of bridge proves itself in actual use capable of withstanding the ordinary accidents of railroading better than this, then it is a safer and a better bridge. The Apple River Bridge on the Chicago, St. Paul and Kansas City Railway illustrates this point very well. This bridge was a single span of 180 feet. It was built with all the refinements known to modern practice. The stresses in all the members were computed to a nicety, and the dimensions of the parts proportioned according to accepted rules. The workmanship and material were subject to rigid inspection and the testing machine added its evidence that the pieces broken in two were of the proper standard. When finished, this bridge might well stand as a good example of what is generally considered the most approved American practice. It did so stand for about one month, when the breaking of an axle under an ordinary freight car suddenly put an end to its usefulness. This axle broke some 3 600 feet from the bridge, and dragged harmlessly over the ties for that distance. But as soon as it struck the open spaces between the bridge ties it stuck, the car to which it was attached "slewed" around, the trusses were hit, and the whole structure fell into the stream below. The breaking of an axle is by no means an uncommon occurrence, and is as likely to happen on a bridge as anywhere else—more likely, some think. The breaking and dropping of a brake-beam or draw-head is another common accident. So is the shifting of a load or the falling off of some article not sufficiently secured on a platform car. I have records of pin-connected bridges wrecked by all of these common accidents, but after years of diligent inquiry I have failed to find a single instance where a riveted bridge fell in consequence of any such accident, though I have a great many records of these accidents happening upon such bridges.

Mr. Cooper asks, "Does any one advocate the designing and building of bridges to withstand the impact of a railroad train or the bursting effect of piling two trains on one another inside the trusses?" I answer, that as long as trains run into bridges or cars pile up inside the trusses, a bridge to be safe must be designed and built to withstand just those things. The Erie Canal Bridge at Tonawanda would have been in a bad way on that January day in 1873, when the train ran into it, had it not been able to withstand the impact. On that day a spar was being drawn across the track at a road crossing near the bridge. It sagged considerably in the middle, and as the railroad track was higher than the road on either side, it "grounded" on the track. Just then a train came around the curve, and, the distance being too short to stop, struck the spar, shoved it along against the bridge and finally came to a stop after shoving the whole span bodily some little distance on the abutments. The pilot, head-light and smokestack of the engine were smashed, but the bridge was not appreciably injured, and after being pushed back

into place continued to be used, and is used to-day, without further repairs. It is a riveted lattice bridge, about 100 feet long. Now, by way of contrast, the 150 feet span, pin-connected, Pratt truss bridge at Fish's Eddy, on the New York, Ontario and Western Railway could not withstand a glancing blow from a derailed caboose which was being dragged along at the tail end of a freight train. This bridge was built under specifications substantially the same as those of the Erie Railroad of 1878, but with a very liberal allowance of material, so that the span was a heavy one. It collapsed under the blow in March, 1886, and dropped into the stream below, with five cars.

If bridges could not be built to withstand the piling up of cars inside the trusses, then the Delaware, Lackawanna and Western Railroad Company's bridge over Wappasening Creek, near Nichols, N. Y., would have come to an untimely end last year when the freight train ran into a preceding coal train upon it. The cars were piled up to two-thirds the height of the trusses, some of the web members broken off, and others badly bent in both trusses. But the trusses stood, because the members were rigidly riveted together, though it is not a particularly good specimen of a riveted bridge either. On the contrary, when the collision occurred between the two sections of a broken Erie freight train on the bridge over the Delaware and Hudson Canal, near Port Jervis, in 1882, the cars piled up and the bridge went down. It was a pin-connected bridge, and considered a good one too.

To say that such accidents are to be classed as failures of management and not of bridges is only begging the question. I apprehend that as long as a bridge goes down it is of little consequence to those most interested, whether they be the people aboard the train or the stockholders who foot the bill, whether the fault lay in the bridge or the management. If a bridge is properly constructed a fault of the management will not destroy it.

In regard to the list of bridge failures, the publication of which Mr. Cooper so strongly deprecates, as I am responsible for the collection of these statistics I may, perhaps be permitted to offer a few statements in their defense.

Mr. Cooper objects to these reports because they "are not based entirely on official reports, but are collated from the newspapers." This is by no means true. In every case where a bridge accident has been brought to my notice I have used every possible means to verify the facts reported. If by "official reports" Mr. Cooper means the reports of any public officials who may have power to investigate accidents, his statement is incorrect, for such reports have invariably been largely relied upon wherever accessible. But Mr. Cooper must be aware that only in comparatively few instances is the failure of a bridge ever made the subject of public inquiry. If he means by "official reports" the reports of railroad employees to their superior officers, then his state-

ment is in the main correct, for a railroad company is not willing, as a rule, to furnish for publication any facts relating to an accident on its own line, especially if due to a defect in its road or equipment, or to a fault of its management. Other sources of information which I have used besides "official reports" are the files of local newspapers very often; photographs of wrecks whenever obtainable; personal correspondence with individuals in the neighborhood in almost every instance, and occasionally the testimony before a coroner's jury. The assertion that these reports of mine are "undoubtedly full of sensation and inaccuracies" is a gratuitous piece of misinformation, and it is to be regretted that such a sweeping charge should be publicly made without at least some attempt to ascertain how much basis there might be for it. I have never claimed absolute accuracy for any such lists of bridge failures, though I have never published as a fact any statement which I had not reasonable grounds for believing to be true, Mr. Cooper's assertion that the "data * * * have been magnified and distorted" to the contrary notwithstanding. In spite of all attempts I have sometimes found it impossible to get any information from any source about certain accidents. In every published list of accidents which ever came from my hands attention has been called to these doubtful cases, and correction and criticism invited. As the result of such invitation, after the list of two hundred and sixty-five failures for ten years ending December 31st, 1888, was published, to which Mr. Cooper refers, I was advised of one error in the case of a wooden bridge reported to have fallen from some unknown cause. This proved to be a trestle, and as the list was not intended to cover these structures the total number of failures was reduced to two hundred and sixty-four. This correction was published in the same journal in which the original list appeared, but seems to have escaped Mr. Cooper's attention.

Mr. Cooper lays much stress on the fact that only ten iron bridges are positively known to have squarely fallen down in the period mentioned. An American iron bridge seldom falls through sheer weakness. It never does except in consequence of some defect of design, manufacture or maintenance, which engineering skill or intelligent inspection could have detected. The most common cause of failure among American iron bridges is a blow either from some projecting part of a passing train or from derailed cars. In the list of two hundred and sixty-four failures referred to sixty were known to have been caused by blows. Of these bridges eleven were of wood, twenty of iron and twenty-nine of unknown construction. If the same proportion between wood and iron holds among the unknown as among the known cases we may reasonably estimate that forty spans out of the sixty which were knocked down were of iron. Moreover, the list includes sixty-five bridges of unknown construction which failed from unknown causes. It is altogether probable that some of these were of iron and were knocked down.

Every one of the twenty iron bridges known to have been knocked down was of the pin-connected type. They were of all ages, from one week to many years. They represent the engineering talent and manufacturing skill of many of the most prominent bridge building establishments in America. Some of them were on roads especially noted for the care displayed in the construction and maintenance of bridges, and were furnished with all the safety appliances known to modern practice. As long as pin-connected bridges continue thus to fall, even though they be of the very best design and workmanship, while riveted bridges have repeatedly stood precisely the same kind of accidents and even worse ones without serious injury, I believe the latter are safer and therefore better. I have never known a pin-connected bridge to stand after its end post had been broken in two, but I know of at least two such cases of riveted bridges. I have never known a pin-connected bridge to stand after being shoved out of place so that one corner was pushed off the abutment, but there are a number of cases on record where a riveted bridge has thus stood "on three legs" as it were.

I believe with Mr. Benjamin Baker that "it is clearly necessary to provide for the contingency of a train accident on a bridge." I believe that the only way we know of as yet to accomplish this result is by using riveted connections, and that the time will come when the failure of an iron bridge from an ordinary accident of train service will be regarded as discreditable to its builder and not excused as a fault of the management. Until that time comes we may expect to see published occasionally lists of bridge failures in which the number of iron pin-connected bridges knocked down will be a considerable item, however distasteful the publication of such lists may be to the builders of those bridges.

In closing his treatise on the theory and practice of bridge building, the late Squire Whipple uses these words: "After all, practical test is generally the only satisfactory means of determining the value and utility of any mechanical device." It is when judged by this criterion that the merits of riveted construction show at their best.

C. L. STROBEL, M. Am. Soc. C. E.—Mr. Cooper's paper contains a fair and full exposition of the merits of the American type of bridge construction. After many years' trial, the American pin-connected truss is as firmly rooted in favor among engineers who have had experience with it, and who are competent to judge it, as it ever was. The arguments which have been advanced in favor of the riveted lattice as against the pin-connected type of truss, are also applicable in favor of the multiple-system pin-connected truss as against the single system. That these arguments have not found favor among bridge engineers is proved by the fact that the tendency is universal to design trusses of both the pin-connected and the riveted types as single systems, thereby reducing the number of parts and increasing the definiteness of strain

in each part. The verdict of engineers has been, that the advantage claimed in favor of a multiple-system truss, viz., that it offers greater security in case of accident to train, is illusory and of minor consequence compared with the counter advantages which a single-system truss offers, the most important of which is, that it is better suited to take care of the normal or every-day service for which a bridge is primarily built.

As Mr. Cooper has pointed out, vibrations can be guarded against in a pin-connected truss so that they will not be greater than in a riveted truss; therefore this feature has little or nothing to do with the comparative merits of the two types.

A properly proportioned pin-joint never gives trouble, but a properly proportioned riveted joint, for which the riveting must be done in the field, frequently does, for the following reasons:

First.—The riveting must be done by hand.

Second.—Field riveters, generally speaking, are inferior to shop riveters in skill. As high a degree of discipline cannot be maintained among the former, and supervision and control are more difficult.

Third.—It is frequently difficult to do field riveting properly because of inadequate space and awkward positions in which the men have to do their work, or the interference of trains or bad weather.

Fourth.—There will be loose rivets, notwithstanding all precautions that can be taken, so much so, that it is a practice much to be recommended, to go over the work after the lapse of a certain time, say a year, to replace these.

The methods for determining the maximum shears and maximum moments for wheel loads in a single system truss described by Mr. Cooper are much older than the year 1880. I have used the method for determining the maximum shears since 1874, but both methods had been taught in German polytechnic schools prior to that. It cannot be successfully maintained that the method for determining the maximum moments is as short of application as the method of equivalent loads. Furthermore, the former methods are not applicable to draw spans, nor to double intersection trusses, still much used and a very advantageous form in some cases, nor to the Petit truss, of late years preferred for long spans. To overcome these difficulties and to shorten the work of computing the strains, I adopted, about the year 1879, a method of calculating the strains in trusses by assuming an "equivalent" uniform load so determined that the end shear (*i. e.*, the vertical component of the strain in the end brace) obtained from it would be the same as the maximum end shear for the actual train load. At that time the engine load generally specified was much heavier than the freight load is, yet it was found that the strains obtained by this method were close approximations to the strains obtained from the train load, except that in the chords of trusses exceeding 150 feet in span, the former method

gave an appreciable excess in the middle panels. This excess is about 5 per cent. for a 200 feet span of eleven panels, Erie train load. To overcome this difficulty I used an equivalent load for the chords of spans exceeding 200 feet made up of the freight load covering the entire span, and an excess load of about the length of two engines of such value, that again the end shear was the same as the maximum end shear for train load.

At the present time the difference between engine load and freight load is very slight, some specifications even prescribing a heavier freight load than engine load. That being the case, the errors resulting from a calculation by the method of equivalent loads, using only one series of loads, are inappreciable, and it is no longer necessary to use excess loads for the chord strains of long spans.

Mr. Cooper's table showing the equivalent loads obtained from center moments, from "double shear" and from end shear, is misleading and erroneous. It applies to Erie train load, which is not the usual train load specified at the present time, and, because the difference between engine and freight load is great, would make, if possible, an unfavorable showing for the method of equivalent loads. The column giving equivalent loads from moments is not correct for spans of 80 feet and over, *i. e.*, for the truss spans. For these spans the loads given in this column correspond to the end shear of trusses having panel lengths of 15 to 18 feet, and not to the center moments. The panel length has, of course, some influence on the value of the end shear, it being greater for short than for long panels. This column appears to be taken from the specifications of the Keystone Bridge Company, where the equivalent loads were determined in two ways, viz., from center moments for plate girder spans (70 feet and under), and from end shears for truss spans (80 feet and over).

Mr. Cooper's "end shears" are apparently the shears next to the support. There is no use for these shears in the calculation of strains for a truss, and for a plate girder. These end shears are used only to determine the thickness of web at the end of the span, also the strength of rivet connections, stringers to floor beams. No one would have difficulty in making the proper allowances to determine these correctly from one series of equivalent loads.

As regards the column of equivalent loads from "double shear," to be used in the calculation of floor beams and trestle columns, the "span" has not been assumed properly. It must be taken equal to two panel lengths for floor beams, and to two spans for a trestle column, *i. e.*, to the distance between columns on either side of the one considered. Making this correction there will be found very close agreement between the values of Mr. Cooper's second and first columns, where before there was great divergence (see the following table), and it follows, therefore, that the method of equivalent loads, using one series only, is also applicable

to the calculation of floor beams and trestle columns, and consequently also to vertical suspenders in trusses.

EQUIVALENT LOADS FOR ERIE TRAIN LOAD.

1	2	3	4	5
Span, Feet.	Number of Panels.	From moments to 70 feet. From single shear for 80 feet and upwards Cooper and Keystone Bridge Company.	From double shear. Cooper transposed. (To be used for floor beams and trestle columns.)	From single shear. No panels. Cooper. (To be used for rivet connections of stringers and to determine thickness of web at ends of plate girders.)
5	....	8 800	.....	9 680
10	....	5 290	5 280	7 200
15	....	5 280	.....	6 240
20	....	4 640	4 620	5 400
25	....	4 400	.....	5 040
30	....	4 200	4 210	4 740
35	....	4 080	.....	4 540
40	....	3 850	3 830	4 300
45	....	3 700	.....	4 220
50	....	3 590	3 588	4 240
55	....	3 450	.....	3 930
60	....	3 400	3 400	3 830
70	....	3 220	3 270	3 680
80	5	3 260	3 175	3 620
90	6	3 250	3 090	3 570
100	6	3 230	3 030	3 450
125	7	3 150 (3 231)	.....	3 280
150	9	3 070 (2 913)	.....	3 180
175	10	2 990 (2 852)	.....	3 090
200	11	2 910 (2 768)	2 750	3 000

The figures in parentheses in third column are the equivalent loads from center moments.

In the above table the difference between the figures in third and in fourth columns show the errors that will be made by calculating floor beams and trestle columns by the use of the equivalent loads given in column 3. For column 4 the panel length is considered as one-half the span length.

Mr. Cooper's method of providing for the camber by a fixed addition to and deduction from the lengths of the chords, is not, in my judgment, sufficiently accurate for all cases. By this method a larger camber is provided to take care of the deflection due to the changes of length of the web members, for a shallow than for a deep truss, whereas the deflection is the reverse, and for trusses of irregular form (chords not parallel) or those of the Pettit order, difficulties are encountered to determine the proper lengths of the web members. For large bridges I have preferred the following method, which is both simple in application for all forms of trusses, and accurate as well.

It is assumed at the outset that the main object of providing camber is to make the truss take its most advantageous form when fully loaded, *i. e.*, when the chords are strained to their maximum. This condition is

attained, for a Pratt truss, when the posts are plumb and the chords are in straight line under full load. The top chord joints will then be closed theoretically, the abutting surfaces bearing squarely upon each other. The camber lengths which will give this condition are obtained by first calculating the lengths of the members for the normal form of the truss, which it is to assume under full load, and lengthening and shortening each member according to the changes of length produced by the strain it receives under this load. The chord strains for full load are always known, as they are the maximum, but the strains in web members for this condition of loading have to be calculated, which is easily done. If the posts and suspension bars are of the same length respectively, it will be desirable to take an average strain per square inch for all the posts, and the same for all the bars, to avoid having these lengths vary slightly according to the different strains per square inch, corresponding to this loading, but for trusses with variable lengths of web members this approximation need not be made.

In conclusion I wish to express my high appreciation of the admirable manner in which Mr. Cooper has treated his comprehensive subject.

J. A. L. Waddell, M. Am. Soc. C. E.—Mr. Cooper's paper is most timely in that it refutes the erroneous conclusions of English engineers as to the alleged inferiority of American railroad bridges in comparison with those of English design and manufacture, which conclusions were drawn from an ill-advised paper read a short time ago before the Institution of Civil Engineers by an American engineer. It would have been much better for all concerned, if that gentleman had presented his views on American bridges to an American society, where his statements and deductions would have received due comment, and where the effect of his paper would have been productive of good; instead of submitting it to those who are only too ready to severely criticise and throw discredit upon the work of their professional brethren in this country. No one is more ready or willing to expose faults in American bridges than I, nevertheless, I contend, and have in the past maintained, that the modern American railroad bridges are the best bridges in the world; not only because they are designed according to strictly scientific principles, but also because they are detailed in a thoroughly practical manner with reference to simplicity, economy and effectiveness.

In comparison with them the bridges of English design, which I have seen and read of, appear to be very crude affairs. The English bridges which were being built three years ago in Japan involve more serious faults of design than do the pioneer iron bridges of the United States, that are now being taken down and replaced by modern structures. And yet these Japanese bridges were designed, or were supposed to have been designed, if the name-plates which they bore were reliable, by one of the most prominent English members of the Institution of Civil Engineers.

That American railroad managers have been so short-sighted and parsimonious as to let bridges by the span in keen competition without employing the services of a consulting engineer, and that many structures in consequence are both light and defective, is no reason for English engineers to claim superiority over their American brethren, when the former, while under no pressure from competition, but on the contrary, with *carte blanche* as to the use of metal, design such structures as those sold to the Japanese Government. Was an American bridge engineer ever guilty of designing a 100 foot span railroad truss-bridge without a single diagonal lateral member in the structure? And yet the standard railroad bridges of Japan three years ago were built in this manner! Nor was this their only fault of design—the trusses were entirely unsupported against overturning, except by small fish-bellied floor beams which were most inefficiently riveted at their ends to the bottom chords. As these beams were only, if I remember rightly, 8 inches deep at their ends, their actual strength when tested to destruction would probably be very low indeed.

The limiting length of span on the Japanese roads under control of English engineers was for a long time 100 feet; but some three years ago there was built over the Karasugawa a bridge with spans of 209 feet; and, *mirabile dictu*, the design for said spans was merely an amplification of the design for the 100 feet spans, except that overhead struts without diagonal laterals were adopted. However, when the drawings were sent to England for approval, the upper lateral rods were put in; but the bridge was built without a single lower lateral rod in its whole length.

The accompanying photograph illustrates very well the character of the structure (Plate XCIII). The truss depth appears to be 16 or 17 feet, which is about what English authorities regard as the economic depth for a girder of this length. American engineers would make such spans fully twice as deep for economy. A comparison of web and chords in the photograph will convince one that most of the metal is in the latter; and it is well known to American engineers that with parallel chords the greatest economy of material is obtained when the weight of the web is about equal to that of the chords. European engineers and technical writers maintain that the economic depth of truss varies from $\frac{1}{8}$ to $\frac{1}{12}$ or even $\frac{1}{14}$ of the span, while American practice shows it to be from $\frac{1}{5}$ to $\frac{1}{7}$ of the span—just about twice as great. Again, English engineers adopt very short panels, while American engineers use long ones. Both theory and practice prove that the latter are superior to the former in respect to every important consideration, viz.: economy of material, rigidity and facility in erection.

Secondary strains due to eccentric action of stresses in both tension and compression members are a very common feature in bridges of English design—not only in short spans but also in the longest and heaviest

manufactured. Professor W. C. Kernot, M. Am. Soc. C. E., of Melbourne, Australia, in *The Engineer* of June 5th, 1885, pointed out how in the curved top chord of an important Indian bridge the intensity of working stress varies from zero on one side to twice the average on the other, thus making the member only one-half as strong as it was estimated.

For quite a while a certain American engineer has been advocating in the engineering papers that the building of pin-connected spans be abandoned, and that lattice girders be used in their stead. Such action would be retrograde and entirely uncalled for, because pin-connected spans can be designed so as to embody all the advantages of riveted girders without any of their numerous disadvantages.

The principal plea of this gentleman is that pin-connected bridges when run into by a derailed locomotive or car will collapse, while the lattice girders, on account of their multiple system of cancellation, may not. But as it is practicable to design all railroad bridges so that the trusses will not be struck by derailed trains, or any permanent part of the structure be injured thereby, it would be folly to adopt a style of truss that had so many objectionable features as has the lattice girder. The most important of these are the following: Ambiguity of stress distribution on the several systems of triangulation, secondary strains due to fixed ends of web members, the use of single angle irons for diagonals, eccentric action of stresses in chord members, unavoidable ambiguity of stress distribution on connecting rivets, excessive amount of field riveting, and in long spans the unnecessarily great dead load caused by loss of section from rivet holes, the great number of parts, and the large amount of what may be termed idle metal.

Bridges can be properly protected against injury by derailed trains in the following ways:

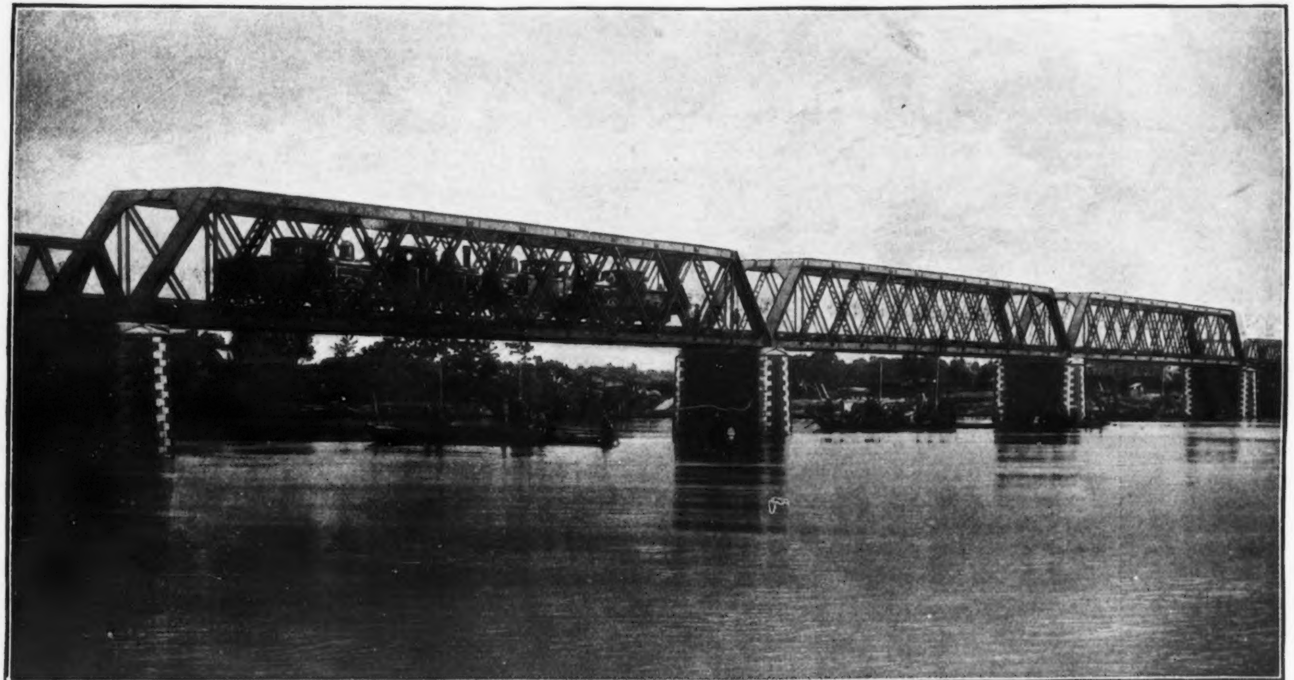
First.—Widening the clear distance between trusses to 16 feet. This would also reduce the danger from shifting loads, such as piles, that occasionally strike the trusses.

Second.—By adopting an efficient design for the floor system proper, (or ties, rails, guards and their attachments).

Third.—By having a re-railing apparatus at each end of every bridge, with ditching apparatus at a convenient distance from each end, also a center rail extending from re-railing to ditching apparatus.

Fourth.—By driving two very heavy protecting piles in the embankment at each end of each bridge, and bracing them effectually, so as to break up or push back any unduly projecting portion of a car load.

General managers and superintendents of railroads are much to blame for the improper styles of floor system used on many American lines. A few extra dollars spent on each span would change a bad floor system into a good one, because a first-class floor system, or rather that portion of it above the stringers, can be built at a cost of four or five dollars per lineal foot of track.



Vol. XXI, p. 598. KARASUGAWA BRIDGE, JAPAN. PLATE XCIII.

All the old iron bridges in present use should be thoroughly examined and investigated by experts; then, if found deficient, as is liable to be the case, repaired or replaced as may seem advisable. I have had occasion to examine a number of iron bridges in the West, which were built fifteen or twenty years ago, and have found them invariably dangerously weak in important details. Unless speedily removed, some of these old iron bridges will surely cause accidents that may prove to be catastrophes.

Railroad managers, in order to insure obtaining really first-class bridges for their roads, should either have complete detailed drawings and specifications concerning quality of material, workmanship, &c., prepared by competent bridge specialists, then let the contracts on schedule prices and engage competent inspectors to look after the material during manufacture and erection; or should call for tenders upon standard specifications, submit the papers of all the competitors to a bridge specialist, and let him award the contract, then have the contractor submit complete working drawings to the expert for approval before manufacture be commenced; and finally, as before, arrange for proper inspection.

It is decidedly bad policy to let bridges by the span, owing to the temptation which the contractor is subjected to in regard to saving metal, and thus scamping the work. On the other hand, if bridges be let by the pound without any supervision by a bridge engineer, they are liable to cost an excessive amount, to say nothing of the chance of some of the details being imperfectly designed.

Every railroad company should either have in its employ a competent bridge engineer, or should retain some specialist as consulting engineer on all its important bridge work.

In order to become a good bridge designer, an engineer should have experience in the employ of both contracting bridge company and railroad company, for with the first he will learn economic methods of design, and with the second he can observe how bridges act under loads, and what are good and what are bad details.

The effect of keen competition upon bridges, even when standard specifications are used, is often prejudicial, in that, for the purpose of saving a little material, the designer will go to extremes, thus impairing some one or more of the attributes of a first-class structure. It is here that the usefulness of the consulting engineer becomes evident; for he would rule out in competition such extreme designs. The various effects of wind pressure are not completely treated in some standard specifications, consequently bidders will naturally take advantage of such omission, although as engineers they know that it is not proper to do so.

My personal experience as contractors' engineer, and conversations that I have had with bridge companies' computers, cause me to differ with Mr. Cooper in respect to the facility of application of the moment

diagram in ascertaining stresses in bridge trusses. For short spans, square bridges and panels of same length throughout, that method is certainly the best; but for long span bridges with sub-divided panels and inclined top chords, skew bridges and spans different from the ordinary, the uniform load method, with or without engine excess, is far preferable. It is most exasperating to a computer to be compelled to try four or five different positions of the moving load in order to ascertain which one will give the absolutely greatest stress on a member of a sub-divided panel truss, when he knows that there will be hardly any difference in the stresses for all the positions which he has to try. With a uniform load and two engine excesses separated by two panel lengths (about 50 feet), one calculation would suffice. It would not do for the computer to guess at which position of the train will give the greatest stress, for the railroad company's engineer might throw out his design from competition on account of not being quite up to specifications.

The method which I have adopted in designing long span railroad bridges is to use the moment diagram for the floor system and primary truss members, an equivalent uniformly distributed load for the chords and inclined end posts, and the uniform car load headed by one engine excess and accompanied by another two panel lengths distant from the first for the main web members. This method reduces the labor of computation to a minimum, and at the same time gives results that are exact enough to satisfy any but a very captious engineer.

Mr. Cooper makes a very good point when he advises that we do not increase our unit strains in proportion to our faith in new material—especially in reference to short spans, which are nearly always made too light for rigidity. Moreover, steel, although stronger, is no stiffer than wrought-iron; and as, for short spans, and the floor systems of long spans, rigidity is even more important than is strength, it would, in my opinion, be well to use therein the same intensities of working stresses for both steel and iron. At the same time, in the trusses of long spans, I am in favor of straining steel as high as good judgment, based upon adequate testing, would indicate.

At the risk of being considered tedious, I would again urge upon the profession the organization of an association of bridge engineers and builders for the determination of complete standard specifications for railroad bridges, and for the testing of new forms of members and new material. Surely the Government of the United States could be prevailed upon to spend a considerable sum in testing not only material for and full-sized members of bridges, but also the complete bridges themselves! We do not yet know enough about the effect of impact upon structures and upon the members of structures, and it is going to cost money to learn it.

Could not the American Society of Civil Engineers persuade the

Government to appoint a commission of three bridge experts at adequate salaries for the purpose of conducting a series of such tests, and to furnish all the money needed for same? In my opinion, it would be well for the Society to give this suggestion consideration.

D. J. Whittemore, Past President Am. Soc. C. E.—The thanks of our members are due to Mr. Cooper for his able presentation of the important subject of which his paper treats.

As indicating that the Mississippi Bridge at Minneapolis, designed by the late C. Shaler Smith, M. Am. Soc. C. E., is one of importance, I may say that it consists of three river spans, the central of which is 324 feet, and the shore spans are each 272 feet, and the grade of track on same is 150 feet above the river.

Probably Mr. Cooper, when he wrote the paragraph in regard to his Erie specifications of 1878, was unaware of the specifications for the Kilbourn and the Rockton Bridges, the first issued in July, 1877, and the last in October of the same year, both esteemed by many engineers as "covering the designing, proportioning and detail of construction with that completeness necessary to give the railroad company the full advantage of the competitive method with a certainty that the resulting structure would in all ways be up to the advanced state of the art."

The Kilbourn Bridge specifications soon after their issuance were made the subject matter of a paper by C. Graham Smith, President of the Liverpool Society of Civil Engineers, which paper was republished by an engineering journal in this country soon afterwards.

A copy of the Kilbourn Bridge specifications is now deposited with the Secretary of this Society.

There were so many calls for the Rockton specifications immediately after their issuance and since, that only one copy remains in my charge, and as it is an official paper, I am unable to part with it.

I presume copies may be found among the archives of many of the principal bridge building firms of 1877. The specifications were largely copied from subsequently.

The author of these specifications was aided materially in their preparation by the late C. Shaler Smith, and whatever merit they possess is largely due to his advice.

Theodore Cooper, M. Am. Soc. C. E.—When the paper under discussion was read before the convention of the Society at Seabright, the author requested errors of fact to be pointed out, that corrections might be made before the final publication. A few minor changes have therefore been made. Where opinions were advanced by the author, criticism is proper and desirable.

The author desires to disclaim any intention of undervaluing the services of many able bridge builders in the advancement of American bridge work from his failure to enter in detail into all the minor steps

of progress. Even were the data obtainable, to have enumerated each kind and variety of structure with the mechanical and structural details peculiar to each, and to have attempted to credit to each able engineer the part taken by him in such advancement, would have been a very difficult and extensive task. He sought therefore to confine his descriptions to the marked stages of development only.

Messrs. Pegram and Strobel in their able discussions differ from the author in his advocacy of engine loads in preference to uniform loads for the specified live loads. They advocate other methods as being fully as correct and of more easy application.

While acknowledging that their methods, differing from each other, may give practically as good results for any particular case as the method advocated in the paper, the author thinks they have neglected to appreciate some important points.

Every experienced computer of bridge strains acquires "short cuts" or methods of approximation which are practically correct in final results. It may be by use of tables of equivalent uniform loads for the various classes of engines, by use of an engine excess, or by a leading concentrated load. Where the judgment of an individual designer only has to be considered, such methods can give as near the maximum strains induced by the traffic of a particular road as any specified engine load.

The objections to the use of such methods for general purposes are as follows:

First.—The methods of approximation employed by different computors are as various as the types of locomotives adopted by other engineers, and an agreement upon any one method would be very difficult.

Second.—Such methods of approximation must always be supplemented by certain corrections in particular cases. Mr. Pegram says: "It is assumed that the uniform load will extend over the panel in front of the concentrated load, and the half panel load at the advanced panel point shall be neglected, a condition in favor of simplicity and involving no error when it is an understood feature of the calculations."

Mr. Strobel says (in calculating floor beams and trestle columns): "It (the span) must be taken equal to two panel lengths for floor beams and to two spans for a trestle column."

Third.—Where competitive plans are submitted the only method of determining the relative merits of the strain sheets is by reference to some positive criterion. For this purpose there is no better or more correct method than a definite engine and train load.

Fourth.—And to the author's mind the most important objection, the use of methods which will be incomprehensible to the officials who have largely the decision in determining the capacity of our bridges. When an engineer recommends to the controlling power of a railroad the desirability of providing for a possible future train load, consisting of locomotives and cars of certain styles and weights, he puts the subject in a comprehensible shape.

On the other hand, the relative merit of a particular uniform load table or a certain excess in the form of a concentrated load cannot be properly appreciated. It must be recognized that as a rule the decision in such matters is not left to the engineer. The average official who has the decision in such matters may be perfectly competent to judge of the necessity and possibility of the adoption of some particular kind and weight of rolling stock for the future, but he cannot grasp the importance of specifying some particular concentrated load in connection with a uniform load to represent the strains induced by such rolling stock.

The methods of providing for the camber given by Mr. Strobel and Mr. Pegram do not appear to be as easy of application nor more correct than the method described by the author.

By increasing the top chord the desired fractional amount in excess of the lower chord, leaving the distances between the chords unchanged and then figuring the proper lengths of the diagonals to correspond, we have a truss in which all the members are of proper length and free from all strain when placed on its side. The method is therefore correct in principle and of very easy application.

Mr. Pegram's assumption that the live loads to be used in the future will increase as rapidly as they have in the past is not likely to be realized. Unless the whole railroad system of the country is radically changed so as to admit cars of a greater carrying cross section than now, there is a definite limit where the loads of the cars per lineal foot of track must stop. The carrying cross section of our freight cars now does not vary far from 55 square feet, and unless they exceed the cross section of a Pullman car, cannot exceed 65 square feet.

Excepting the metals and heavier minerals and a limited number of special articles, the freight transported by our railroads will not exceed in weight, as usually stored, 50 pounds per cubic foot.

This weight per cubic foot will cover nearly all merchandise, all food and grain products, steamer compressed cotton and bale goods, coal, coke and lumber—a classification that will cover a very large percentage of all our freight, and, in many systems where the heavy ores are not handled, the total traffic. It is not likely that the heavier ores and the metals will ever be loaded to any large fraction of the carrying cross section of our present cars.

The maximum even for such material is likely to be limited by the loads upon the wheels and rails, and will not probably exceed what is found as suitable from engine wheel loadings.

It therefore seems to the author that the loads per lineal foot of our maximum trains will not be advanced very much or very rapidly beyond 4 000 pounds.

The "100 000 pounds" car of the Pennsylvania Railroad weighs 51 800 pounds and is 40 feet long over all, which is equivalent to 3 795 pounds per lineal foot.

Special cars may be built which will exceed the limit of 4 000 pounds,

but it is not probable that any system of cars exceeding this limit will be adopted for some years, if ever.

That any cars can ever be used which will produce a greater strain on any part of our bridges than our present locomotives, is not believed by the author. It is also very certain that however heavy the cars may become the engines will keep advancing in nearly equal proportion.

Morever, for the great mass of bridges on our roads, spans less than 150 feet, the maximum strains must be produced by two heavy engines —an ordinary possibility.

Mr. Pegram also suggests providing for the effects of impact by adopting certain relations between the uniform and concentrated loads advocated by him.

The author considers a far better method, and one covering other desirable features, is to use different unit strains for the dead and live loads.

Mr. Waddell renews a suggestion that is popular with certain engineers, the establishing of standard specifications by some authority, either a technical or governmental one. The author cannot agree to any such idea. The great and rapid progress in the past has been without any such means, and at no time in the past could any such act have been performed without a distinct blocking of the evolution and progress of the science of bridge building. Are we prepared to say that we have now reached perfection and need no longer the conflict of various opinions to produce further advancement? A standard would not mean the best result even of our present knowledge on any subject, but a compromise to suit the wishes of the majority—a result of the average intellect, and not the belief of the most advanced or experienced leaders.

The experience of the world has been ages older in the use of other materials than iron or steel, and yet engineers have never agreed upon any standards for other constructions, masonry, wood or earth.

We may each individually believe we know all the knowledge in regard to our specialties, but we have no right to force other minds fully as strong in their faith to accept our ideas as the ultimate.

The specifications named by Mr. Whittemore and Mr. Stowell, as of prior claim to the Erie were unknown to the author. On examination he still thinks they are not such full and complete general specifications as the Erie, although they were excellent ones for that day. They certainly were not known sufficiently to have exerted that influence for advancement that has been credited to those of the Erie Railway. The author, however, did not wish to convey the idea that he received no aid or assistance directly or indirectly from the experience of other engineers in preparing these specifications. He gladly availed himself of the advice and criticisms of some of the best of our bridge engineers. To the late C. Shaler Smith, M. Am. Soc. C. E., to whom the advancement of American bridge building owes so much, he was especially indebted for numerous valuable suggestions.

Mr. H. B. Seaman, "from the several specifications in his possession," finds himself justified in questioning the assertion of the author as to American bridge practice in estimating the lateral forces. He also decides the motive to be, not what the author has asserted in his paper, but "to lessen labor and to facilitate the manufacture of so many tons of iron in a certain limited time." He does not make clear, however, how a different method of estimating the lateral forces for preparing a strain sheet is going to aid the very desirable purpose of facilitating the manufacture.

It would appear from the tone of the discussion upon lattice bridges that some of the advocates for this class of structure have thought injustice has been done to this type by the author. He has had no intention to do other than justice, and has put forth fully his reasons for any judgment or opinion expressed. He disclaims any prejudice against this type. He has acknowledged the excellent character and good record of bridges which have been built upon this type. He is further ready to acknowledge the great improvement of our pin-connected bridges by the adoption of features in use in lattice bridges, especially such points as tend to greater rigidity of the structure, as stiff laterals, rigid connections of the floor beams, etc. He, however, has stated that all the good points of lattice bridges can be obtained by proper designs of the pin-connection form.

Lattice bridges have also been influenced and improved by features originally peculiar to the pin-connected bridge, long panels and the greater concentration of parts.

The essential features of the two types, nevertheless, remain distinct, and upon these points the relative merits of the two systems must be determined. The critical difference between the two systems does not consist in any special forms of the individual members or the methods of connecting the floor beams or braces, but in the methods of connecting together the essential members of the trusses; and, as a necessary result of these connections, the different degrees of accuracy in determining the effective lengths of the members and the correctness of the fittings of the joints. When Mr. Stowell talks of the importance of the connection of floor beams and stringers, he is entirely begging the question. The relative merits of the two systems is in nowise dependent on these connections. The accuracy of all parts of the pin-connected bridge is determined at the shop by the most accurate tools and instruments. The joining of them together during erection is simply performed by driving in the accurately turned pins. When so connected the designer can feel the strongest assurance that his design has not been ruined by any reaming of holes or imperfect riveting,

The accuracy of the lattice bridge is dependent upon, 1st, the correctness of the hand-made templet; 2d, the correctness of the marking of the iron from these templets; 3d, the correctness of the punching, and,

4th, the faithfulness of the workmen during erection in bringing all the members to a perfect fit before driving the rivets. Then finally the perfection of the joint depends upon the perfection of the riveting, —usually done by hand on temporary scaffolding.

That good and satisfactory bridges have been and are made in this manner is a credit to our bridge shops and workmen. That such joints have been considered good enough by the workmen before they accurately coincided or have been so incorrectly made that reaming was necessary, is also undoubtedly true.

It is also true, as related by Mr. Stowell, that lattice bridges have been swung clear of the supports by the temporary use of bolts and drift pins. While an emergency would justify such a proceeding, the strongest advocates of the lattice system would hardly consider this as a desirable condition in which to make the permanent connections by riveting.

Mr. Bush candidly acknowledges that "the best type of pin-connected bridge is superior to any riveted bridge in spans over about 125 feet, or, say, 100 feet in double track bridges."

An experienced builder of lattice bridges has stated to me that his experience has confirmed him in the belief that lattice bridges should not be built exceeding 180 feet.

As plate girders are rapidly cutting off the lower limits and the upper limits are being lowered even by those experienced with the lattice type, it can hardly be credited to the author as a prejudice if he has ventured to analyze the reasons for American engineers preferring certain types over others. The author has never deprecated the publication of bridge accidents, as Mr. Stowell says, but he does object to the collection and publication of sensational and prejudicial accounts of accidents for any partisan purpose, and when such publications are made by one holding an official position, the author desires most distinctly to deny that such reports are to be considered as having in any manner the weight of "official reports" or what would be considered in foreign countries as unbiassed and carefully digested reports based upon an official examination into all the facts.

Mr. Stowell is fully entitled personally to the freedom of any hobby he may wish to indulge in. But he must blame no one for weighing any testimony presented by him in the light of his prejudices. Mr. Stowell states: "I have records of pin-connected bridges wrecked by all of these common accidents (breaking of an axle, etc.), but after years of diligent inquiry I have failed to find a single instance where a riveted bridge fell in consequence of any such accident." Mr. Stowell must have known of the St. George Bridge accident on the Grand Trunk Railway, February 27th, 1889, as the account was fully published in the *Engineering News* of March 9th. From the breaking of a tire the train entered the bridge derailed; one span and part of the train fell. The bridge was a lattice bridge. The failure was in no manner due to the

character of the trusses, but was a fault of management in having a defective floor system. Mr. Stowell's list consists largely of pin-connected bridges destroyed for precisely similar reasons. Why does he make an exception of this case? Are faults of management on pin-connected bridges only to be classed as bridge failures?

The author again asserts that such compilations of bridge accidents have been magnified and distorted and are undoubtedly full of sensation and inaccuracies.

He also regrets to believe that when the law of probability has its full effect, the number of iron bridges over 100 feet span being as 6 500 for pin-connected trusses to 500 (Mr. Stowell's estimate) for lattice bridges, Mr. Stowell's faith that lattice bridges cannot be split open or knocked down by colliding or derailed trains or other faults of management will be severely shocked.

In regard to the publication of accidents to or upon bridges, the author advocates the fullest records regardless of any bias or prejudice for or against any style of bridge. But such records should be classed in the proper manner, faults due to the type, if such there be, faults due to the details or construction of the structure, faults of maintenance or operation. The purpose of such publication should be the truth and a desire to improve the safety of our railroads.

It should also be borne in mind that the bridge designer or builder has little voice in the methods to be employed for guarding or protecting a bridge. This point devolves upon the maintenance of way authorities.

It is therefore harmful to class all accidents of management as bridge accidents and to put any part of the fault upon the style of the structure which belongs not to the bridge engineer but to the train or track management. The remedy will be much sooner applied by making clear the true fault and placing the blame on the proper authorities.

AMERICAN SOCIETY OF CIVIL ENGINEERS.

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TRANSACTIONS.

NOTE.—This Society is not responsible, as a body, for the facts and opinions advanced in any of its publications.

430.

(Vol. XXI.—December, 1889.)

DISCUSSION

ON

THE SIBLEY BRIDGE.*

By H. V. HINCKLEY, M. Am. Soc. C. E. and

W. H. BREITHAUPT, Am. Soc. C. E.

H. V. HINCKLEY, M. Am. Soc. C. E.—The following statement of prices and cost of the Sibley Bridge is interesting, as also the cost of the Blair and Plattsmouth bridges, which I give here for comparison :

COST EXCLUSIVE OF 3 553 FEET OF TRESTLE, EAST APPROACH, PROPERTY OF RAILWAY COMPANY.

SUPERSTRUCTURE.		
3 spans, 396 feet each.		
Iron, 1 545 684 pounds, at 4¼ cents†	$65 691 57	
Steel, 2 156 252 pounds, at 4½ cents†	97 031 34	
		$162 722 91
1 span, 247 feet.		
561 082 pounds iron and steel.		
1 span, 197 feet.		
371 295 pounds iron and steel.		
2 spans, 172 feet.		
504 516 pounds iron and steel.		
1 436 893 pounds of iron and steel, at 4.45 cents†		63 941 74

* Transactions, Vol. XXI, No. 422, September, 1889.

† Erected and painted two coats.

VIADUCT.

1 793 882 pounds, at 4.32 cents*	$77 495 70
False work as per contract (same stated therein)	23 000 00
For changing height of viaduct	997 03
	$328 157 38
Miscellaneous	8 614 55
Total superstructure	$336 771 83

SUBSTRUCTURE.

Pier No. 1.

Excavation	$277 79	
Concrete, 156.14 cubic yards, at $6.50	1 014 91	
Masonry, 103.57 cubic yards, at $14.75	1 527 66	
Coping, 13.84 cubic yards, at $20	276 80	
Granite bridge seats, 1.78 cubic yards, at $49	87 22	
		$3 184 38

Pier No. 2.

Excavation	$500 91	
Concrete, 383.96 cubic yards, at $6.50	2 495 74	
Masonry, 974.46 cubic yards, at $14.75	14 373 28	
Coping, 59.84 cubic yards, at $20	1 196 80	
Granite bridge seats, 7.78 cubic yards, at $49	381 22	
Rip-rap, 212.61 cubic yards, at $2	425 22	
		19 373 17

Pier No. 3.

Timber in caisson and crib, 251 036.209 feet, at $40	$10 041 45	
Wrought-iron, 31 509.0 pounds, at 4½ cents	1 417 90	
Cast-iron, 1 301.0 pounds, at 4 cents	52 04	
Sinking, 53 947.575 feet, at 18 cents	9 710 56	
Bowlders, 19 329.75 feet, at 35 cents	6 765 41	
Concrete in air chambers, Portland, 46.0 cubic yards, at $15.50	713 00	
Concrete in air chambers, American, 466.62 cubic yards, at $14	6 532 68	
Concrete in crib, 262.644 cubic yards, at $6.50	1 707 19	
Main shaft left in caisson, 32.2 lineal feet, at $10	322 00	
Masonry, limestone, 1 554.16 cubic yards, at $14.75	22 923 86	
Masonry, granite, 156.25 cubic yards, at $35	5 468 75	
Coping, limestone, 69.47 cubic yards, at $20	1 389 40	
Granite bridge seats, 7.34 cubic yards, at $49	359 66	
		67 403 90

Pier No. 4.

Timber in caisson and crib, 241 968.0 feet, at $40	$9 678 72	
Wrought-iron, 29 143.0 pounds, at 4½ cents	1 311 44	
Cast-iron, 790.0 pounds, at 4 cents	31 60	
Sinking (cubic feet displacement), 52 608.0 feet, at 18 cents	9 469 44	
Bowlders (taken out through air lock), 22 796.8 cu. feet, at 35 cents	7 978 88	
Concrete in air chamber, Portland, 30.0 cubic yards, at $15.50	465 00	
Concrete in air chamber, American, 388.81 cubic yards, at $14	5 443 34	
Concrete in crib, 333 451, at $6.50	2 167 43	
Main shaft left in caisson, 30.75 lineal feet, $10	307 50	
Masonry, limestone, 1 511.96 cubic yards, at $14.75	22 301 41	
Masonry, granite, 156.25 cubic yards, at $35	5 468 78	
Coping, limestone, 69.47 cubic yards, at $20	1 389 40	
Granite bridge seats, 7.34 cubic yards, at $49	359 66	
		66 372 57

Pier No. 5.

Timber in caisson and crib, 238 653.0 feet, at $40	$9 546 12	
Wrought-iron, 27 061.0 pounds, at 4½ cents	1 217 75	
Cast-iron, 1 247.0 pounds, at 4 cents	49 88	
Sinking, 51 142.10 feet, at 18 cents	9 205 58	
Bowlders, 21 393.37 feet, at 35 cents	7 489 43	
Concrete in air chamber, Portland, 28.0 cubic yards, at $15.50	434 00	
Concrete in air chamber, American, 322.03 cubic yards, at $14	4 508 42	
Concrete in crib, 546.52 cubic yards, at $6.50	3 552 38	
Main shaft left in caisson, 39.9 lineal feet, at $10	399 00	
Masonry, limestone, 1 369.01 cubic yards, at $14.75	20 192 90	
Coping, limestone, 65.92 cubic yards, at $20	1 318 40	
Granite bridge seats, 6.78 cubic yards, at $49	332 22	
		58 246 08

* Erected and painted two coats.

Pier No. 6.

Timber in caisson and crib, 167 051.01 feet at $40	$6 682 04	
Wrought iron, 18 988.00 pounds at 4½ cents	854 46	
Cast-iron, 1 438.00 pounds at 4 cents	59 52	
Sinking, 43 444.86 feet at 18 cents	7 820 07	
Bowlders, 10 080.00 feet at 35 cents	3 528 00	
Concrete in air chamber, 195.64 cubic yards at $14	2 738 96	
Concrete in crib, 393.45 cubic yards at $6.50	2 557 42	
Main shaft left in caissons, 31.95 lineal feet at $10	319 50	
Masonry, limestone, 659.06 cubic yards at $14.75	9 721 14	
Coping, limestone, 32.25 cubic yards at $20	645 00	
Granite bridge seats, 6.48 cubic yards at $49	317 52	
		$35 241 13

Pier No. 7.

Timber in caisson and crib, 117 422.00 feet at $40	$4 698 88	
Wrought iron, 12 170.0 pounds at 4½ cents	547 65	
Cast-iron, 947.0 pounds at 4 cents	37 88	
Sinking, 33 274.80 feet at 18 cents	5 989 46	
Bowlders, 5 184.0 feet at 35 cents	1 814 40	
Concrete in air chamber, 147 737.0 cubic yards at $14	2 068 32	
Concrete in crib, 272.69 cubic yards at $6.50	1 772 48	
Main shaft left in caisson, 45.0 lineal feet at $10	450 00	
Supply shaft left in caisson, 7.0 lineal feet at $4	28 00	
Masonry, limestone, 503.09 cubic yards at $14.75	7 420 58	
Coping, limestone, 15.77 cubic yards at $20	315 40	
		25 143 05

Pier No. 8.

Excavation	$1 648 77	
Timber in cofferdam, 362 940 feet at $40	1 451 76	
Concrete in cofferdam, 48.89 cubic yards at $6.50	317 78	
Wrought iron in cofferdam, 1 165 pounds at 4½ cents	52 42	
Cast-iron in cofferdam, 508.0 pounds at 4 cents	20 32	
Piling, 3 360 lineal feet at 50 cents	1 680 00	
Timber in grillage, 15 282 feet at $40	611 28	
Timber used second time, 1 282 feet at $25	32 05	
Wrought iron in grillage, 1 479 pounds at 4½ cents	66 56	
Cast-iron in grillage, 30 pounds at 4 cents	1 20	
Masonry, limestone, 418.45 cubic yards at $14.75	6 172 14	
Coping, limestone, 15.07 cubic yards at $20	301 40	
Rip-rap, 378.32 cubic yards at $2	756 64	
		13 107 32

Pedestal Piers.

Excavation	$2 496 13	
Concrete in foundation, 1 775.58 cubic yards at $6.50	11 543 22	
Masonry, limestone, 669.71 cubic yards at $16	10 715 36	
Coping, limestone, 73.90 cubic yards at $20	1 478 00	
Dowel holes, 1 018.0 lineal feet at $1	1 018 00	
Anchor plates fitted and set, 84 at $2.50	210 00	
Timber left around concrete, 2 280 feet at $40	115 20	
Rip-rap, 3 609.78 cubic yards at $2	7 219 56	
		34 795 46
		$322 867 57
Miscellaneous		29 758 43
Total substructure		$352 656 00

Summation.

Superstructure	$336 771 83
Substructure	352 626 00
Surveying and engineering	24 062 84
Miscellaneous, such as shore protection, etc	62 692 11
Grand Total, November 30th, 1888	$776 151 78

Blair Crossing Bridge over the Missouri River. Erected 1882–83. George S. Morison, Chief Engineer.

Superstructure.

	Steel. Pounds.	Wrought Iron. Pounds.	Cast-iron. Pounds.
Three through spans, each 330 feet	884 548	1 447 148	57 322
One deck span on west end, 176 feet	6 753	238 550	4 325
One deck span on east end, 110 feet	4 979	110 503	8 955

Substructure.

(Pier No. 1 is at West End of 330 Feet Span.)

	Pier No. 1. Feet.	Pier No. 2. Feet.	Pier No. 3. Feet.	Pier No. 4. Feet.	Other Masonry.
Height of pier (masonry), about	74	100	100	74	......
Top of pier to high water, about	45	49	49	45	......
Depth of water, about	4	7	32	13	......
Top of pier to bottom of caisson, about	112	117	117	112	......
Cubic yards, masonry	1 081.7	1 562.4	1 578.9	1 108.4	5.8
Cubic yards, concrete	1 198.7	589.4	592.3	1 205.4	252.3

Cost of foundations of piers Nos. 1, 2, 3 and 4	$94 364 69
Cost of masonry of piers Nos. 1, 2, 3 and 4	125 963 62
Cost of approach piers	6 495 88
Total	$226 824 19

Summary.

	Exclusive of Freight.	Freight.	Total.
Superstructure	$200 336 68	$6 188 80	$206 525 48
Substructure	190 383 39	36 440 80	226 824 19
Approaches	181 655 97	9 044 35	190 700 32
Protection works	214 238 73	187 462 08	401 700 81
Tracks	45 915 56	1 073 99	46 989 55
Buildings and tools	17 547 01	639 18	18 186 19
Real estate	9 112 76	....	9 112 76
Engineering	27 051 54	....	27 051 54
	$886 241 64	$240 849 20	$1 127 090 84

Plattsmouth Bridge across the Missouri River at Plattsmouth, Neb. Erected 1879–1880. George S. Morison, Chief Engineer.

Superstructure (Commencing at west end).

120 feet iron viaduct, trestle	61 529 pounds iron.			.... pounds steel.		
2 spans of 400 feet double intersection, through trusses	702 706	"	"	983 703	"	"
3 spans of 200 feet, single intersection, deck plate trusses	800 871	"	"	10 290	"	"
1 440 feet iron viaduct	799 557	"	"	....	"	"
Total	2 364 663	"	"	993 993		

2 000 feet temporary trestle, afterwards filled, connected with east embankment.

Cost of Superstructure.

Two 400 feet spans	$114 436 12
Three 200 feet spans	35 039 76
Viaducts	36 236 06
Floor and painting	19 457 82
	$205 169 76
Freight charges	16 079 31
Total	$221 249 07

The two 400 feet spans are across the deep water. Highest water during 1877-1880 = 511 and lowest 495 (±).

The three 200 feet spans cross the overflowed land.

Substructure.

(Pier No. 1 is at west end of first 400 feet span.)

	Pier No. 1.	Pier No. 2	Pier No. 3.	Pier No. 4.	Pier No. 5.	Pier No. 6.	West of No. 1.	East of No. 6.
Elevation top of masonry	559.0	559.0	559.0	529.5	528.5	527.5	......	......
Elevation bottom of masonry	495.0	495.0	491.5	497.5	496.5	505.5	......	......
Elevation bottom of concrete	461.5	....	....	....	493.7	502.5	......	......
Elevation bottom of caisson	......	464.2	444.3	442.9	....	....	....	......
Cubic yards masonry	800.16	781.04	786.34	257.97	258.98	141.30	345.5	541.5
Cubic yards concrete	987.2	945.4	1 467.2	965.7	58.4	50.9	44.7	195.0
Cost { Foundation without freight	$16 758 47	$32 567 97	$45 461 92	$36 522 23	$3 945 57	$626 83	$12 517 43	
Cost { Masonry	14 450 57	15 154 92	16 164 45	4 829 94	3 776 06	1 903 85	....	
Total, freight inclusive.	39 607 23	56 613 86	71 597 48	47 075 85	9 785 40	2 892 02	13 184 40	

Total Cost.

Substructure	$240 156 24	
Superstructure	221 249 07	
		$461 405 31 bridge proper.
East approach	$33 942 48	
West approach	59 295 44	
		93 237 92 approaches.
Permanent track		16 209 35
Shore protection		1 611 71
Buildings, tools and temporary tracks		10 526 90
Engineering and incidentals		26 950 54
		$609 941 73

W. H. Breithaupt, M. Am. Soc. C. E.—Mr. Scherzer's point* as to compression due to wind in bottom chord of 400-foot spans is well taken. Wind pressure will, with the improbable combination cited, give resultant compression strain in third panel of bottom chord, and such strain might even be greater than that shown, as a train of empty box cars may run under 1 000 pounds per running foot in weight. Opposing it are transverse rigidity of floor beam flanges (12½ inches wide with cover plate on each) up to outer stringer, and eventually friction and anchor bolts at pedestals, together offering resistance with unit strains well

* Transactions, September, 1889, p. 130.

within elastic limit of material in rare cases of occurrence of above strain.

Obviating the bending of intermediate posts when span deflects, as it does in its regular use under passing loads, by enlarging pin holes where diagonals cross as described in the text, must be considered of greater importance than provision by means of this central pin against torsion of post due to eccentricity of lower lateral connections, such torsion being provided against by bottom chord pin, as lower laterals are directly above bottom chords.

The end stiffening strut is fixed to both end post and intermediate post, being riveted to each.